

# *The American Journal of SEMITIC LANGUAGES AND LITERATURES*

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The Egyptian Middle Kingdom at Megiddo	<i>John A. Wilson</i>	225
The Origin of the Name "Hebrews" - - -	<i>E. G. Kraeling</i>	237
The Date of the Byblos Temples	<i>Robert J. Braidwood</i>	254
Women and the State on the Eve of Islam	<i>Nabia Abbott</i>	259
Persian and Egyptian Chronology - -	<i>Richard A. Parker</i>	285
An Aramaic Journal Page - - -	<i>Raymond A. Bowman</i>	302
Darius and Xerxes in Babylonia - -	<i>George G. Cameron</i>	314
Two Inscriptions of Ashurnasirpal - - - -	<i>Tom Jones</i>	326
Book Reviews - - - - -		327
Walter C. Klein, <i>Al-Ibānah ḥan Uṣūl ad-Diyānah</i> (L. V. Thomas)		

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THE EGYPTIAN MIDDLE KINGDOM AT MEGIDDO

JOHN A. WILSON

An extreme simplification of the history of ancient Egypt might confine itself to action and counteraction in the play of forces between Egypt and its neighbors. Thus the late predynastic age showed strong Asiatic influences coming into the land of the Nile. Then the Old Kingdom exploited Sinai, Phoenicia, and perhaps Palestine economically. In the First Intermediate Period Asiatics "invaded" the Egyptian Delta. The Middle Kingdom moved again into Asia in some measure and with some authority. The Second Intermediate Period saw the Hyksos invasion of Egypt. The New Kingdom set up an Egyptian empire in Asia. The balance swung again with attempted invasions of Egypt in the thirteenth and twelfth centuries B.C., etc. The scheme of things becomes clearer in these later phases; the nature of the empire under the New Kingdom is fairly well known. We know less about the outreaching of the Old and Middle kingdoms into the areas beyond their normal frontiers. What was the nature of Egyptian "imperialism" under the Middle Kingdom?

The Middle Kingdom did not spring into being fully armored. It took time for the pharaohs of the Eleventh and Twelfth dynasties to establish their authority within Egypt. When that was accomplished they were ready to reach out toward regions beyond the frontiers. Sesostris III established his authority solidly at the Second Cataract, and trading-posts reached as far south as Kerma near the Third Cataract. Was there a similar situation in Syria-Palestine?

Oriental Institute A 18622 (Pls. I-III) is the lower portion of an Egyptian seated statuette, found by the excavations of the Oriental

Institute at Megiddo in Palestine. Gordon Loud, field director of the Megiddo Expedition, makes the following statement on the discovery of the statuette (Field No. A 1199) out of its normal context:

This and fragments of three uninscribed statuettes constitute an Egyptian group found obviously out of place in and about the Stratum VIIB phase (13th–12th century B.C.) of the eastern temple, which itself is contemporary with the Late Bronze Age palace. Three of these pieces, including the one under discussion, were incorporated into the rubble of which the temple platform was built. The fourth had reached an equally low status in the rubbish supporting the pavement just outside the temple door. In material they vary from the coarse, gray, granite-like stone of the one found outside the temple to a fine, black, polished stone, possibly diorite (e.g., see *Illustrated London News*, June 20, 1936, p. 1108, Fig. 2, where—as also in A. Rowe, *A Catalogue of Egyptian Scarabs . . . in the Palestine Archaeological Museum*, p. xlvi—the names on our statuette are incorrectly given on the basis of preliminary study).

Investigation has shown that the foundations of the temple originated not later than Stratum VIII (15th–14th century B.C.) and were so deep-set that they rested at one point no more than a meter above a standing wall of Stratum XV (20th–19th century B.C.), reused in XIV (19th century B.C.), to which level or levels we should attribute these statues. It is not improbable, therefore, that they were first encountered in the excavation for the foundations of the temple, and being of a useful building material were thus incorporated in the structure of the building.

The extant fragment of the statuette is approximately nine and a half inches high (24 cm. high by 17 cm. long by 13.5 cm. wide). The material is a hard, black stone of the diorite or basalt type. The individual depicted was represented as seated upon a chair, with his left palm upon his left knee and his right hand holding a kerchief the ends of which are visible against his right thigh. He wears a pleated skirt with a folded forepiece. Musculature visible on the left leg suggests a sculptor of ability.

Four columns of hieroglyphic inscription cover the left side of the base and four columns the right. The supporting column at the back of the statuette probably once ran up to the individual's head, with a single column of text. The inscriptions present conventional appeals for a certain Thuthotep, with little more than names and titles. The following translation must be provisional, as certain broken passages and certain titles puzzle me. Fortunately, the essentials of the inscriptions are clear. I am indebted to Dr. Keith C. Seele for collation, correction, and corroboration.

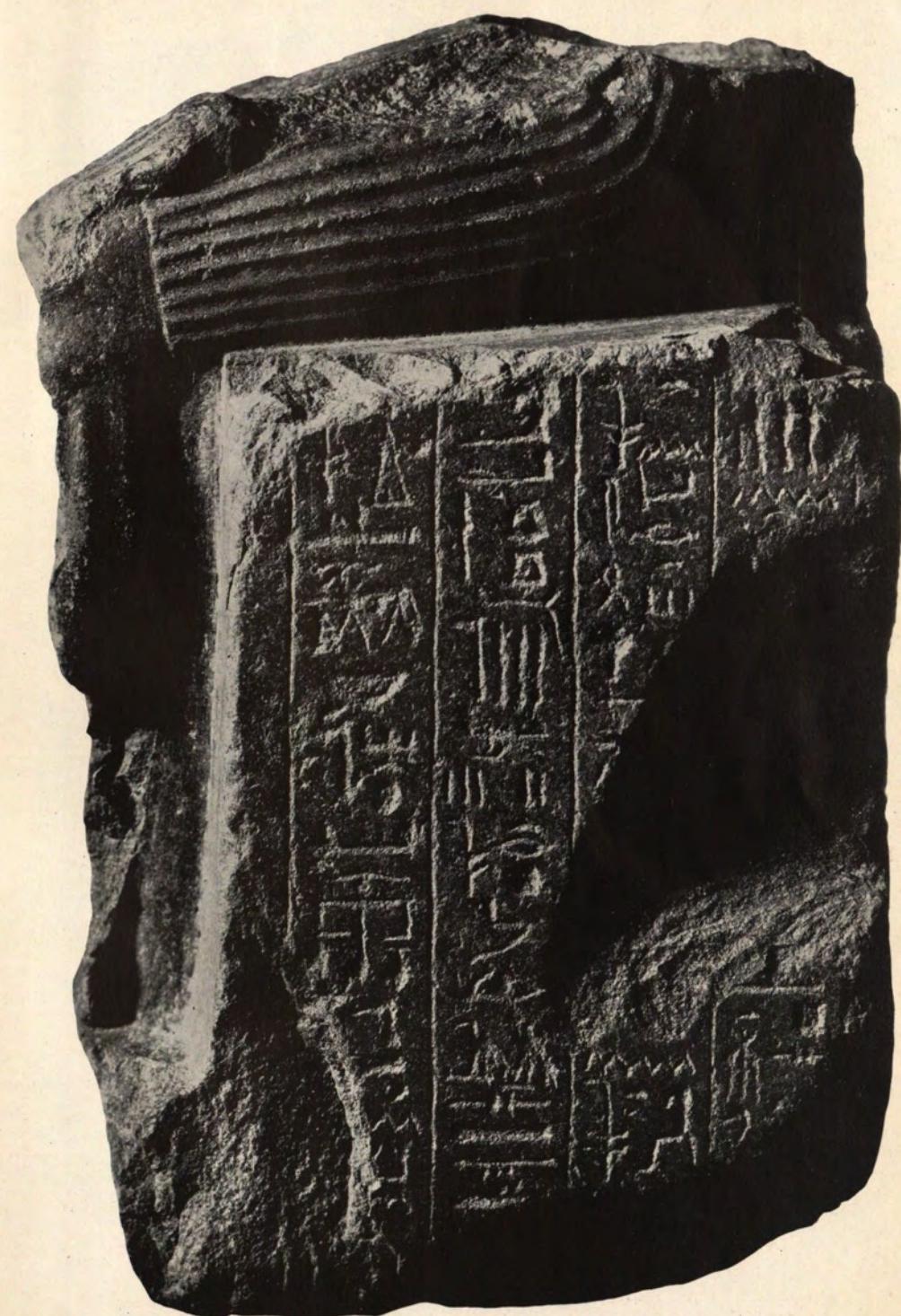
PLATE I



B

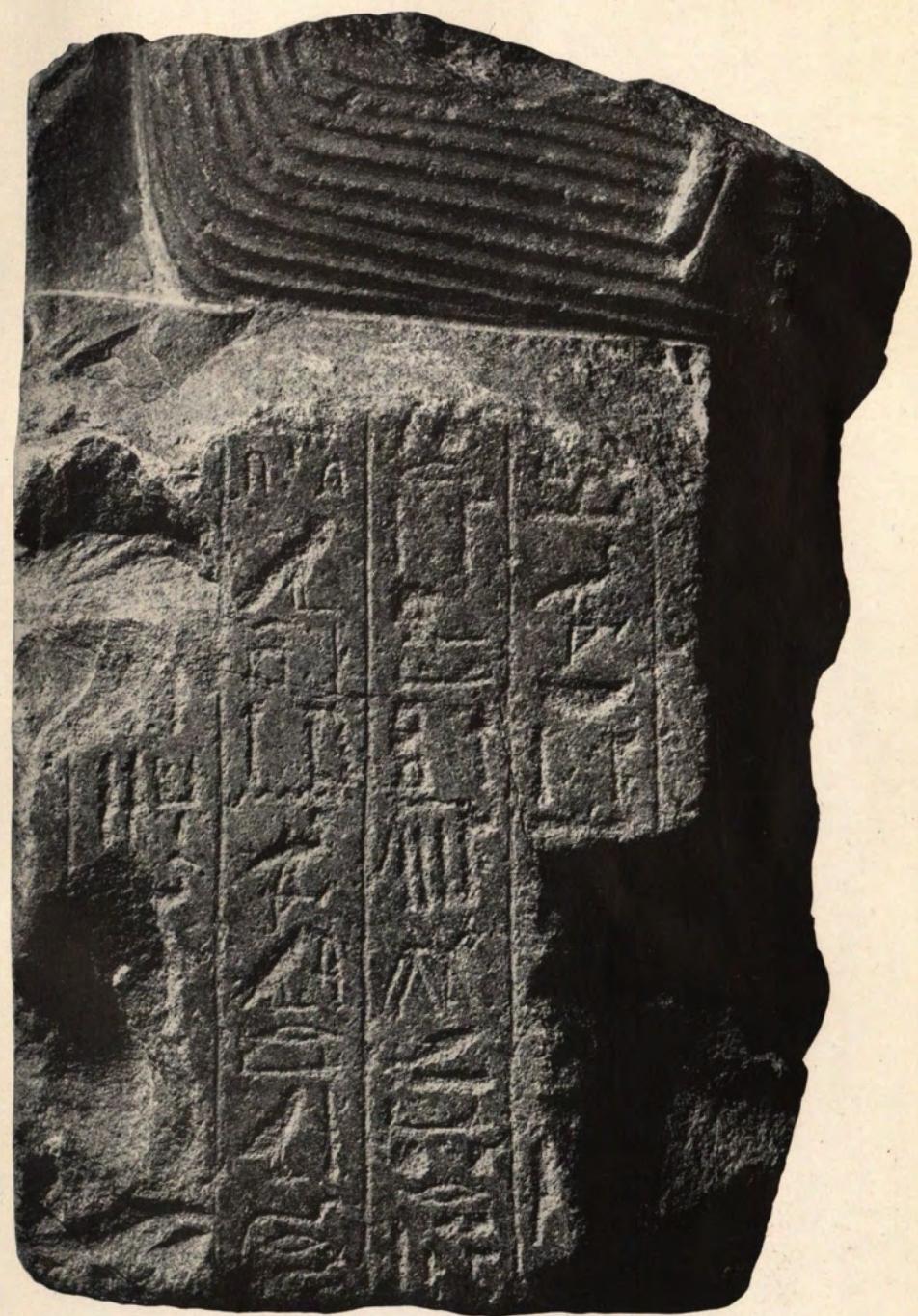
A

EGYPTIAN STATUETTE FOUND AT MEGIDDO, GENERAL VIEW AND REAR



EGYPTIAN STATUETTE FOUND AT MEGIDDO, LEFT SIDE

PLATE III



EGYPTIAN STATUETTE FOUND AT MEGIDDO, RIGHT SIDE

## TRANSLATION

## LEFT SIDE

- (1) An offering which the king gives (to) Khnum, Lord of the Foreign-Country-of-the-God,<sup>(a)</sup> that he may give an invocation-offering (consisting of) bread, beer, [cattle], fowl, etc.,<sup>(b)</sup> to the *ka* of the 'revered one'<sup>(c)</sup> (2) the Count, Controller of the Two Thrones, Overseer of Priests, Chief of Five,<sup>(d)</sup> Royal Intimate, he who sees the mysteries [of] . . .<sup>(e)</sup> (3) the King and exalts the courtiers, the Great Overlord of [the Hare Nome], . . . 'of the Royal Favorite . . .<sup>(f)</sup> (4) at the head of<sup>g</sup> . . . , [Thut]hotep, born to Si[t-Kheper-ke].<sup>(g)</sup>

## RIGHT SIDE

- (1) An offering which the king gives (to) Thoth, Lord of Divine Words,<sup>(h)</sup> . . .<sup>(i)</sup> (2) 'the one revered in the presence of' the great god,<sup>(j)</sup> the Count, Controller of the Two Thrones, Overseer of Priests, Magistrate and Administrator of Buto,<sup>(k)</sup> 'Mouth of Hierakonpolis, priest . . .<sup>(l)</sup> (3) . . . twenty . . . in the palace<sup>l</sup>,<sup>(m)</sup> High Priest of Thoth, 'sem-priest'<sup>(n)</sup> who has the text read,<sup>(o)</sup> (4) . . . ,<sup>(p)</sup> Kay's son, 'Thuthotep',<sup>(q)</sup> . . .

## REAR

. . . 'in the House of Thoth,<sup>(r)</sup> Great of . . . ,<sup>(s)</sup> Ruler of . . .<sup>(t)</sup> in the House of Khnum, Kay's son, Thuthotep.

## NOTES ON THE TRANSLATION

(a) "The-Foreign-Country-of-the-God" is unknown to me. We have here a sanctuary of the ram-god Khnum. The outline of the signs as they stand on the stone is beyond doubt. There is a question as to whether the land sign may be read *b3st* ("foreign country") or *zmyt* ("highland" or "desert"), but the implications would be essentially the same for the contrast of the upland foreign regions to the flat Egyptian plain. More important is the use of the city-sign determinative, involving the question whether this sanctuary can be in or out of Egypt.

On the face of it, the city sign should mean a town in Egypt, a hitherto unknown sanctuary of Khnum. That may be the safest hypothesis, but I do not wish to discard the possibility that this may be an early and hitherto unknown designation of Megiddo, with a local ram-god of pastoral Palestine assimilated to the Egyptian god Khnum.

In defense of the rare possibility that a non-Egyptian town might

be written with the city sign as determinative, I point to the Empire writing of Beth-Shan so determined, in "Mekal, Lord of Beth-Shan" (A. Rowe, *Topography and History of Beth-Shan*, Pl. 33), and to a Nineteenth Dynasty writing of the name Nebet-Kepen, "Lady of Byblos," with Byblos so determined (Turin 166, *Recueil de travaux*, IV, 140). It must be admitted that this determinative for Asiatic names is highly exceptional. The Empire permitted itself to determine the names of Nubian towns with the city sign, as in the case of Buhen (D. Randall-MacIver and C. L. Woolley, *Buhen*, Text, *passim*) and of Anibeh (G. Steindorff, *Aniba*, Text, *passim*). It may well be that cities which were assumed to be Egyptian, as belonging integrally to the Egyptian empire, might be so treated, especially when an Egyptian deity was lord or lady of the town. If so, Megiddo is here a possibility, and the question of the type of Egyptian control of the town comes into play.

A related problem, also not susceptible of solution, concerns the identity of the god in the name "Foreign-Country-of-the God." One thinks of the analogy of the Egyptian term *t3-ntr*, "Land of the God," which applied to regions east of Egypt, including Syria-Palestine, and in which the god was the sun-god (Ch. Kuentz, *Bulletin de l'Institut français d'archéologie orientale*, XVII, 178 ff.). But it is not necessary that the two names be psychologically related. The "god" may have been the ruling pharaoh. Further, Khnum in the Nineteenth Dynasty had a sanctuary at or near Elephantine called *š.w-ntr*, "Lakes of the God," and on this analogy may himself be the god in question (A. Mariette, *Abydos*, Vol. I, Pl. 45, No. 29).

We emerge from the discussion without established results. We may have here a new name for Megiddo, indicating Egyptian control. It is safer to assume that we have a name for a sanctuary in Egypt, otherwise not yet attested.

(b) No certain trace of the ox's head of the word "cattle" could be isolated. Further, the oval sign under the goose's head may be abbreviated to the right of center. There is some abnormality in the usual formula. See the next note.

(c) The apparent spacing of the *ka*-sign to the right of center is inexplicable. The traces of *im3b*, "revered," at the end of the line are probable but not certain.

(d) Thuthotep's titles "Controller of the Two Thrones, Overseer of

Priests, Chief of Five," as well as the "High Priest of Thoth" (right side, l. 3), are those of the High Priest of Thoth at Hermopolis (R. Anthes, *Zeitschrift für aegyptische Sprache*, LIX, 100 ff.; see also K. Sethe, *Amun und die acht Urgötter von Hermopolis*, §§ 73 ff.). The restoration of "the Hare Nome" in line 3 is certain from Thuthotep's tomb at el-Bersheh.

(e) About half a square is lacking at the end of the line. The translation is grounded on P. E. Newberry, *El Bersheh*, Vol. II, Pl. VII, pp. 23 f.: *m33 s3t3 . . . ny-swt, sds[r] šnywt*, where the writing fortunately brings the word *šnywt*, "courtiers," beyond question. Newberry's restoration of *pr*, "house," in the lacuna, resulting in the translation "he who sees the mysteries of [the House of] the King," seems to be an unsupported guess, although a reasonable one.

(f) A diligent search of Middle Kingdom titles has provided no resolution of the titles or epithets of Thuthotep at the end of this line and at the beginning of the next. A full square is lost at the end of line 3.

(g) The mother's name is known from Thuthotep's tomb. The *t* over the back of the duck cannot be established with certainty. The remainder of the line provides just room for the remainder of the name. For the writing of *ms*, "born," with horizontal *s*, see J. Polotsky, *Zu den Inschriften der 11. Dynastie*, page 21.

(h) Is this the earliest noted occurrence of the epithet of Thoth, *nb mdw-ntr*? A. Erman and H. Grapow, *Wörterbuch der aegyptische Sprache*, II, 181, gives it as beginning in the Eighteenth Dynasty.

(i) The presence of a reed leaf toward the end of the line makes restoration of the customary mortuary formula difficult here.

(j) At the beginning of the line the *im3b* sign is probable but not certain. The *c3* sign in "great god" is certain.

(k) Note the abnormal writing of *Dp*, "Buto," with the *d3* sign.

(l) The arrangement of signs and the visible traces at the end of the line are puzzling. If this be *r3 Nhn*, "Mouth of Hierakonpolis," why is the *Nhn* sign to the right of center? The last group shows the top of a *ntr* sign, followed by a stroke (lower end visible), followed by a tall sign with rounded top. If this be *hm-ntr*, "priest," the writing is extraordinary.

(m) The "twenty" seems to stand isolated, with a possible, but improbable, horizontal trace faintly visible above it. In any case, "thirty" (for the title *wr mdw šm<sup>c</sup>w*, "Chief of Southern Tens") seems

difficult to introduce here. The initial sign of the assumed *ȝzt*, "palace," is not certain.

(n) A clean-looking cut diagonally across a poorly cut *s* makes the reading *sm* uncertain. But what else could it be?

(o) This *rdȝ mdw drf*, later common as an epithet of Thoth, may be a characterization of humans in the Middle Kingdom (K. Sethe, *Historisch-biographische Urkunden des Mittleren Reiches*, I, 62:17; 18:19, occurring in the latter shortly before the title *sm*). For *d* used for *d*, cf. Polotsky, *op. cit.*, page 21.

(p) Unintelligible traces are visible near the top of the line and just above the *hkȝ* of "Kay."

(q) The beak of the Thoth ibis and the right end of the *htp* sign may be discerned, chiefly because one knows where to look for them.

(r) Perhaps "[Chief of Five] in the House of Thoth." The peculiar writing with *m* last might be justified on the theory that the Thoth sign, carrying with it the house sign, had been given honorific precedence. But the uncertainty about the following titles makes any hypothesis difficult.

(s) The first of two unknown titles, which may be only one title. The striding bird looks most like the *gm* bird ("Great of Finding"? "Chief of those who Find the Two *hkȝ* Cords in the House of Khnum"?). But there are other possibilities for the bird, such as *snm*, "feeding"; *hkȝm*, "fishing-fowling"; or *dȝsr*, "red."

(t) On the face of it, *hkȝ rkwy*, "Ruler of the Two Cords." Could it possibly be "Ruler of the Last Two Days (of the Month)"? And one notes a late priestly title of Hierakonpolis and elsewhere, *hkȝ rk* (Erman and Grapow, *op. cit.*, III, 172). This and the preceding title probably involve priestly functions in some sanctuary of Khnum.

#### GENERAL CONCLUSIONS

This, then, is the statuette of the Egyptian official and priest Thuthotep, the son of Kay and of a woman whose name begins Si. . . . From the names and titles there can be no doubt that this Egyptian was Thuthotep, nomarch of the Hare Nome, whose tomb at el-Bersheh is well known.<sup>1</sup> In his tomb we have the statement that he

<sup>1</sup> P. E. Newberry, *El Bersheh*, Vol. I: *The Tomb of Tehuti-hetep*; K. Sethe, *Historisch-biographische Urkunden des Mittleren Reiches*, I, 44 ff.; J. H. Breasted, *Ancient Records of Egypt*, Vol. I, §§ 688 ff. The tomb is famous for the scene showing the transportation of a colossus from the quarries. The Hare Nome was the fifteenth nome of Upper Egypt, with its chief city at Hermopolis Magna, across the river from el-Bersheh.

was a "royal child" under Amenemhet II (1938–1903 B.C., Breasted) and was still an active functionary under Sesostris III (1887–1849 B.C., Breasted). His father Kay invested him with the office of nomarch of the Hare Nome. His mother's name was Sit-Kheper-ke.

It is safe to take it for granted that this small piece actually belonged in Megiddo at some time during Thuthotep's life. But what was a statuette of Thuthotep, high priest of Thoth at Hermopolis and nomarch of a nome in Middle Egypt, doing in Megiddo? I assume that he was resident there in some capacity. It is improbable that he was a member of a merchant colony. His titles and the indications of his career under three successive pharaohs make it unlikely that he was an exile like Sinuhe. An ambassadorial post is a distinct possibility, although we do not know enough about Egyptian envoys at this time to affirm that a man of the rank and responsibilities of Thuthotep would be sent as ambassador to a town like Megiddo.

Other Middle Kingdom objects will be summarized below. Some pieces found in Asia carrying the names of Egyptian individuals may be argued to be those of merchants. But this statuette and that of Sesostris-enekh at Ras Shamra give me the impression of an Egypt which posted men of commanding authority abroad. If so, I must revise my ideas about the foreign relations of the Middle Kingdom. I have assumed that its Asiatic imperialism was commercial and cultural but not military and administrative. But the presence of Thuthotep at Megiddo, added to other accumulating evidence, makes me feel that I have been wrong. Perhaps the Middle Kingdom, in Asia as in Nubia, did extend its administrative control beyond the frontier of Egypt and thus by power held the main arteries of trade.

#### SOME MIDDLE KINGDOM CONTACTS WITH ASIA

The following notes are not intended to be exhaustive. I enumerate some of the more significant objects or blocks of material bearing on the problem of Egyptian relations with Syria-Palestine under the Middle Kingdom. I can draw no binding conclusions from this material; I do draw a strong presumption in favor of a type of Egyptian empire.

A. The biographical evidence on Thuthotep himself is slight, and his tomb provides no evidence that he resided outside of Egypt. An isolated title is *ȝ n bȝst nb(t)*, "Door of Every Foreign Country,"

(Newberry, *op. cit.*, p. 16). I have not noted this title elsewhere. Does it connote frontier or customs or consular responsibility? Newberry's Plate XVIII provides A. M. Blackman with an interesting suggestion in *Journal of Egyptian Archaeology*, II, 13 f. In this scene "cattle of Retenu (Syria-Palestine)" are addressed with the words: "(Once) you trod sand, (but now) you walk on herbage." Understanding that the cattle have been moved from Asia, Blackman points out that this may be indirect evidence of Egyptian military campaigns into Syria-Palestine. On the basis of our statuette these Asiatic cattle may be given further consideration.<sup>2</sup>

B. A stela now in Manchester mentions Sesostris III's conquest of an Asiatic country named *Skmm* (Shechem?), "together with the wretched Retenu" (T. E. Peet, *The Stela of Sebek-khu*; Breasted, *op. cit.*, §§ 676 ff.). This may have been a raid, a conquest of territory, or a punitive expedition against rebels within the Egyptian domain. No other direct statement of Middle Kingdom military activity in Asia is known.<sup>3</sup>

C. The presence in Egypt of *‘3mw*, "Asiatics," as traders or as slaves need not indicate anything but peaceful relations with foreign lands. There is the famous scene of the arrival in Middle Egypt of thirty-seven Asiatics bringing eye paint (P. E. Newberry, *Beni Hasan*, Vol. I, Pls. XXX-XXXI). There are passing references to the transfer of Asiatics within Egypt as slave property (F. Ll. Griffith, *Hieratic Papyri from Kahun and Gurob*, 12:10-11; 13:15-17; 30:35) and to Asiatic dancers at Egyptian feasts (*ibid.*, 24:4-6, 13-14).

Equally inconclusive is such evidence as the Asiatic nature of some of the Tôd treasure of Amenemhet II ("Tôd [1934 à 1936]," *Fouilles de l'Institut français du Caire*, Vol. XVII, Pls. XV-XVII, pp. 113 ff.). Trade or royal gift is a sufficient answer, without assuming tribute.

D. On a wider scale there is a variety of evidence covering an extended period. There was the incursion of Asiatics into the Egyptian

<sup>2</sup> Corroborative evidence from a different tomb group of the Middle Kingdom may lie in the scene of cattle labeled as the "cattle of the Asiatics (*‘3mu*), brought from . . ." (or "brought as . . ."), discussed with caution by Blackman, *The Rock Tombs of Meir*, II, 18 n. In each case the cattle may have come to Egypt by trade rather than conquest.

<sup>3</sup> But note the reported Twelfth Dynasty blocks at Karnak bearing names of Palestine tribute-bearers (R. M. Engberg, *The Hyksos Reconsidered*, p. 33, n. 38). In the same note, the twenty ships of cedarwood sent by Amenemhet I "to meet the Setetyu"-Asiatics rests on a dubious reading; K. Sethe, *Historisch-biographische Urkunden des Mittleren Reiches*, I, 12, reads the destination of these ships as probably "Upper Egypt."

Delta in the First Intermediate Period, and there is the construction of the "Walls of the Ruler" on the eastern frontier (probably a chain of fortresses), built by Amenemhet I "to ward off the *Sttyw*-Asiatics and to crush the Sand-Farers" (Sinuhe, R43). The Middle Kingdom execration texts, whatever their precise date, indicate a very real threat to the Egyptian throne, active relations with Asiatic towns, and a fairly detailed knowledge of the personnel of such towns.<sup>4</sup>

The Middle Kingdom relations with Sinai, for the exploitation of the mines there, are not conclusive evidence on the nature of Egyptian imperialism. For example, in one case Amenemhet III dispatched an expedition of 734 soldiers to the Sinai mines (Breasted, *op. cit.*, § 713). So large a force of soldiers was not necessary from a military standpoint to guard the mines against the Beduin. Rather the soldiers were used as the active Egyptian workers on the mines, just as Ramses IV later sent 5,000 soldiers to the Wadi Hammamat quarries (*ibid.*, Vol. IV, § 466). In other words, such a force does not set the effective Egyptian frontier in Sinai.

Further, no general consideration of the period would be complete without an understanding of the gradual crystallization of the Hyksos movement, beginning before the Twelfth Dynasty and culminating after the Middle Kingdom power had disintegrated.<sup>5</sup>

E. Egyptian scarabs and seals found in Palestine and Syria must be brought into the picture (see especially A. Rowe, *A Catalogue of Egyptian Scarabs . . . in the Palestine Archaeological Museum*). They may be treated in two classes: those containing the titles and names of Egyptians resident in Asia and those containing the titles and names of Asiatic princes. Thus there is the "Scribe of the Vizier, Senebef," found at Jericho (Rowe, S.5), and the "Guard of 110 Asiatics, User-

<sup>4</sup> K. Sethe, *Die Achtung feindlicher Fürsten, Völker und Dinge, etc.* For more recently found documents cf. W. F. Albright, *Bulletin of the American Schools of Oriental Research*, No. 81 (1941), pp. 16 ff. On the basis of my own attempt to enter the hieratic signs of the *Achtungstexte* into Möller's *Hieratische Paläographie*, Vol. I, I am inclined to agree with W. F. Edgerton (*JAOS*, LX, 492, n. 44) that these texts "cannot be earlier than Sesostris III and are more probably to be placed in the Second Intermediate Period." Since their date cannot be fixed with precision, they may be used for this argument only in a general way: Asiatic princes ruled Asiatic towns, but the contacts with Egypt were arguably more direct than those permitted by mere trade and diplomatic relations.

<sup>5</sup> In Engberg, *op. cit.*, and in Albright's studies in *Journal of the Palestine Oriental Society*, VIII, 223 ff.; XV, 194 ff., there are useful treatments of Egyptian-Asiatic relations in the periods in question.

khepesh," of which the Palestine provenience is unknown (Rowe, No. 15—but is it actually of the Twelfth Dynasty?). From Syria come the "Lady of the House, Sit-User" (*Syria*, VIII, 85 ff.) and the "Hereditary Prince and Count, Impi" (*ibid.*). These arguably had temporary posts in Asia. On the other hand, the scarabs of the "Counts of Byblos" carry the Asiatic names *Intr*, *Ibšmu*, and *Ypšmuib* (*Syria*, X, 12 ff.; *Kemi*, I, 90 ff.; *Journal of Egyptian Archaeology*, XIV, 109; XIX, 54). These Asiatics ruled Byblos as princes. But the repeated title *ḥty-*<sup>c</sup>, conventionally translated "Count," is of importance. *In Egypt this title was conferred upon an individual by the king.* (Cf. Breasted, *op. cit.*, Vol. I, § 385, or cf. Hepzefi's inability to transmit property from his count's estate in *ibid.*, § 551.) In other words, even though we do not know that the same situation applied outside of Egypt, this title presents the argument that the Asiatic rulers of Byblos were confirmed in their rule by the king of Egypt, implying a measure of Egyptian control.<sup>6</sup>

To carry on the evidence, at the end of Rowe's catalogue there is a summary graph of Egyptian objects found in Palestine. Ignoring difficulties and taking the evidence in its sweep, this graph provides the following rough ratio of materials: Middle Kingdom, 3; Second Intermediate Period, 7; New Kingdom, 10; and post-New Kingdom, 3. Such a ratio does not argue for an Egyptian empire in the Middle Kingdom of the same intensity as in the New Kingdom. On the other hand, it does indicate a moderately active beginning for the spread of Egyptian influence in Asia on the basis of physical evidence.<sup>7</sup>

F. We come next to objects of Egyptian sculpture bearing hieroglyphic inscriptions and found on Asiatic soil. Inscriptions of the

<sup>6</sup> W. F. Edgerton objects that I overstate my case. The title "Count" was conferred by Egyptian kings, when those kings were strong enough. Perhaps such strength did not reach Byblos under the Middle Kingdom. Edgerton points out that the use of the uraeus to ornament a Byblos scimitar and of the cartouche (turned backward) around a princely name do not suggest that these princes were "loyal," even though they may have been friendly. Such words of caution are necessary in our present knowledge of these times. However, such considerations do not modify my argument, which is not a sweeping claim of an aggressive imperialism.

<sup>7</sup> The materials listed above are not all of the Twelfth Dynasty; some of them belong to the period following that dynasty. In this summary survey I forbear to list uninscribed materials such as pottery or such scarabs as do not contain clear names. It would be instructive to deal with such evidence. See, e.g., Table V in P. L. O. Guy and R. M. Engberg, *Megiddo Tombs*, pp. 190 f., for a survey of foreign relations as indicated by the material found in the tombs at Megiddo.

"Butler Heka-ib" and of the "Citizen Dedu-Amon" were found at Gezer (R. A. S. Macalister, *The Excavation of Gezer*, II, 311 ff.). A sphinx of Amenemhet IV was found at Beirut (*British Museum Quarterly*, II, 87 ff.; *Syria*, IX, 300). A sphinx of the princess Ita, daughter of Amenemhet II, was found at el-Mishrifeh = Qatna (*Syria*, IX, 10 f.). From Ras Shamra come a sphinx of Amenemhet III (*Syria*, Vol. XIV, Pl. XV opp. p. 120), a statuette base of Khnumit-neferhedjet, the wife of Sesostris III (*Syria*, Vol. XIII, Pl. XIV, p. 20), and the statuette group of the vizier Sesostris-enekh (*Syria*, Vol. XV, Pl. XIV opp. p. 116, and pp. 131 ff.). The latter is a most important document. Commenting on the phrase found on this monument, "[to whom was given] the gold of honor," Breasted points out that this was a reward for distinguished service *abroad*.<sup>8</sup> The vizier of Egypt might reside in an important foreign town as an ambassador or as a governor. Perhaps these terms draw too much from modern situations, and we might think of Sesostris-enekh as an Egyptian high commissioner, keeping a watchful eye on a Syrian state which was nominally independent but subject to Egyptian "protection."

There is no need to linger over objects bearing the names of Amenemhet III and IV from the tombs of the princes of Byblos (P. Montet, *Byblos et l'Égypte*, Text, pp. 155 ff.). These were royal gifts to friendly or loyal princes, a remark which applies also to the sphinxes listed above.

On a different footing are two statuettes found in Anatolia, that of the "Nurse Sit-Snefru," found at Adana (*Bulletin of the Metropolitan Museum of Art*, XVI, 208 ff.), and that of an untitled Kerey, found east of Ankara (*AJSL*, XLIII, 294 ff.). At such a distance we can hardly think in terms of firm imperial control. Sit-Snefru may have been an Egyptian governess engaged by some Anatolian prince, and Kerey may have been a merchant. Their presence so far from the land of the Nile is a measure of the range of Egyptian cultural influence under the Middle Kingdom.

On the fragment of an Egyptian ivory wand found at Megiddo the inscription invokes magical protection "over the Lady of the House

<sup>8</sup> *Syria*, XVI, 318 ff. I cannot follow Breasted in his restoration "who satisfies the king as his deputy [in Kha]ru and in the royal cabinet." This would involve a strange spacing and lack of determinative for *ḥrw*, "Syria."

*'B<sup>3</sup>twmw'*, by night during the night and by day during the day" (*Illustrated London News*, November 25, 1939, p. 795). This was found in Stratum VIII (fifteenth-fourteenth centuries B.C.) but in such a context that it may have been carried over from earlier levels. Such magic wands may be of the Eighteenth Dynasty but are most common in the Middle Kingdom. The assumed ♀ sign in the lady's name is highly doubtful, but the name seems to be non-Egyptian in character.

G. Finally, we turn to the story of Sinuhe, a political refugee, who fled from the sphere of Egyptian control at the death of Amenemhet I. The geography of his flight and sojourn in Asia is not clear, but he went as far north as Byblos on the Phoenician coast and then apparently cut to the east, where he was received by a prince of Upper (mountainous) Retenu, in a land of fruits, vines, grains, and cattle. Although he lived near a route which saw the passage of travelers to and from Egypt, he was out of the reach of Egyptian police or legal power. Such a region as the Buqaa, with the great road running north and south between the Lebanon and the Anti-Lebanon, might serve the purposes of the story.

In this case we might suggest that the Egyptian effective control applied to Palestine and to Phoenicia with greater force than to the hinterland of Syria. Or we may have an early Twelfth Dynasty situation, before the later pharaohs had been able to set up a tighter control of Asia. I incline to the latter as a working theory.

No clear-cut conclusions have been won. However, the delegation of the vizier Sesostris-enekh to residence in Ugarit and of the high priest of Hermopolis Thuthotep to residence in Megiddo is significant. This was something more than cultural and commercial empire. If men of such standing were sent to posts in Asia, there must have been a measure of administrative and military empire. On the basis of present evidence a working theory would be that Egypt in the nineteenth century B.C. confirmed the rule of local princes but held them in control by resident high commissioners, possibly backed by garrison troops. Perhaps Egypt of the nineteenth century A.D. provides an analogue for Egypt's Asiatic empire in the nineteenth century B.C.

ORIENTAL INSTITUTE  
UNIVERSITY OF CHICAGO

## THE ORIGIN OF THE NAME "HEBREWS"

E. G. KRAELING

Since the discovery of the Amarna letters much ink has been spilled over the *Ha-bi-ru* and their relationship to the Hebrews.<sup>1</sup> Unfortunately, the problem of who the former were has become more complicated all the while. Since the discoveries of Chiera it has become increasingly clear that the word is not ethnic but rather has something to do with the sphere of social legislation.<sup>2</sup> The vexing problem of the correct transliteration of the cuneiform word *Ha-bi-ru*<sup>3</sup> as *Habiru* or *Hapiru*, and the identity of the Semitic root (whether חָבֵר,<sup>4</sup> עָבֵר, עָבָר) seems now to have been decided by the Ras Shamra texts. Viroilleaud announced on June 30, 1939, that these texts proved that the transliteration should be *Ha-pi-ru*.<sup>5</sup> In a letter to the present writer<sup>6</sup> he quoted the passage on which this claim was based:

*du Hal-bi amelati SAG-GAZ = Hlb cprm*

It associates the Syrian city of *Halbi* with the *SAG.GAZ* people (usually written *SA.GAZ*, another name for the *Ha-bi-ru*) and transcribes this ideogram into Ugaritic *cprm* (עַפְרָם), thus showing that the root is עַבְרָי. In a letter to J. W. Jack<sup>7</sup> several other references to *Halbi* are

<sup>1</sup> The most circumspect of the numerous discussions of the problem prior to the discovery of the Nuzi materials was that of Böhl, "Kanaanäer und Hebräer," *Beiträge zur Wissenschaft vom Alten Testamente*, Vol. IX (1911). It is still worth reading, as are the basic remarks in Knudtzon, *Die El Amarna Tafeln* (1915), pp. 46 f.

<sup>2</sup> Chiera, "Habiru and Hebrews," *AJS*, XLIX (1932/33), 115 f. To this must be added Speiser, "Ethnic Movements in the Near East in the Second Millennium B.C.," *AASOR*, XIII (1933), 13 f., and Lewy, "Habiru and Hebrews," *Hebrew Union College Annual*, XIV (1939), 587 f., where the new source materials are subjected to further amplification and examination. In addition, cf. *ibid.*, XV (1940), 47 f. A good résumé is given by Meek, *Hebrew Origins* (1936), pp. 6 f., where additional literature will be found cited.

<sup>3</sup> The sign *bi* stands for both *bi* and *pi*; the special sign for *pi* alone occurs only by exception in the Amarna letters. Cf. Böhl, *Die Sprache der Amarnabriefe* (1909), p. 9a n. My spelling *Ha-bi-ru* thus emphasizes the ambiguity of the middle syllable.

<sup>4</sup> This was the etymology adopted in my *Aram and Israel* (1918), p. 34. It was recently favored also by Dhorme, *Revue biblique*, 1924, pp. 12 f., and, with changed interpretation, in *Revue de l'histoire des religions*, 1938, pp. 170 f.

<sup>5</sup> Académie des Inscriptions et des Belles-Lettres, *Comptes rendus*, May-June, 1939 p. 329.

<sup>6</sup> Kraeling, "New Light from Ugarit on the Habiri," *BASOR*, LXXVII (February 1940), 32 f.

<sup>7</sup> Jack, "New Light on the Habiru-Hebrew Question," *PEQ*, July, 1940, p. 97.

cited, and Goetze<sup>8</sup> draws the conclusion that they all refer to certain parts of the city—the last *Hlb cprm* being the quarter where the Hapiru, a distinct class of the population, were living.

The question has, however, been raised whether the reading carries the weight that Virolleaud (and myself in the article aforementioned) have attributed to it when we considered it as proving that the connection with the name "Hebrews" was impossible. Albright expressed doubts in a postscript to my article and toned down its final sentence so that, as Rowley<sup>9</sup> observes, it ends on a less certain note than that on which I began it; and Jack, too, expresses doubts in quoting his letter from Virolleaud. It is to be noted that Jack approaches the subject with the firm conviction that there is a relation between the Hebrews, on the one hand, and the *Ha-bi-ru*, on the other, so that it is for him only a question of explaining the divergent Ugaritic reading *cprm*. He makes much of the confusion or interchange of *b* and *p* in the languages, dialects, and scripts of the ancient Near East; a somewhat similar line is taken by Rowley, who, however, is more open minded with respect to the general problem at issue. Their approach here is rather "external," and many of these examples prove little or are themselves uncertain. Of decisive importance, of course, would be the claim that the Ras Shamra texts also spell *cprm* as *cbrm* if that were true.<sup>10</sup> This rests on Virolleaud's communication, some years ago, of the following passage from an unpublished text:

*k ksp l:cbrm zt  
hrs l:cbrm ks*

"Like silver is the olive tree to the *cbrm*  
Gold is the *ks* (-tree) to the *cbrm*."

But Virolleaud's earlier identification of the *cbrm* with the *Ha-bi-ru* (and the Hebrews) in this passage was hasty.<sup>11</sup> There is no reason here to expect the name of a people or a social group. The only natural rendering is "wanderers," "passers-by" (cf. Ps. 129:8, etc.), for one can readily see how a fruit tree would be very desirable to wanderers.

<sup>8</sup> Goetze, "The City Khalbi and the Khapiru People," *BASOR*, LXXIX (October, 1940), 32 f.

<sup>9</sup> Rowley, "Ras Shamra and the Habiru Question," *PEQ*, July, 1940, pp. 90 f.

<sup>10</sup> *Ibid.*, p. 93, n. 5; Jack, *op. cit.*, p. 100.

<sup>11</sup> Virolleaud, "La Mort de Baal," *Syria*, XV (1934), 317; *La Legende de Keret* (1936), p. 74.

whereas there is no reason why it should be more precious to the "Hebrews" than to anybody else. In view of the agreement of Ugaritic, Egyptian (Eperu), and doubtless also the Assyro-Babylonian that the consonant is *p*; in view, furthermore, of the fact that Ugaritic often preserves words more closely corresponding to the primitive Semitic form than Hebrew does, it would seem difficult to defend Jack's hypothesis that the Ugaritic form with *p* is secondary. The shakiness of his position is even further revealed by his introduction of a secondary line of argument. He suggests that the Ras Shamra scribe may have been an Egyptian or have stood under Egyptian influence because he observes a widespread Egyptian inclination to write *p* for *b*. But the rule in Egyptian is just the opposite,<sup>12</sup> and the exceptions he lists are few and, owing to the possibility of errors of hearing on the part of Egyptian scribes, do not prove much. In the case of the Eperu (people who were present in Egypt itself)<sup>13</sup> we may assume that the writing is accurate and does not reflect an error of hearing. At all events it would seem that Jack's conclusion that "in view of the fresh light from the Ras Shamra texts there seems to be decided proof at last that the *Habiru* were Hebrews" is without foundation.

Far more valid than Jack's approach is that of Albright, already mentioned.<sup>14</sup> Here there is no trifling with the fact that *Hapiru* is primary. For him the new reading vindicates a suspicion that he had entertained previously that "*Habiru*" or "*Hebrew*" derives from an earlier *Hapiru*, because stop sounds may become voiced in the presence of *r* (or, as Goetze suggests, only in immediate contact with *r*). Of course, if it is now to be accepted that the word is to be read *Hapiru* wherever *Ha-bi-ru* occurs in the Amarna texts and elsewhere, the change from *p* to *b* would have taken place in the period when South Canaanitic vocalism was modified, leading from *cApiru* > *cIpr*. But, while such a change is a possibility, it can certainly not be said to be a necessity, since it did not occur, for example, in the name of the Midianite clan *Epher* < *Ipr* < *Apir*, long since (e.g., Böhl) considered as a possible equivalent of *Ha-bi-ru* (if it was to be read *Hapiru*), or in words like

<sup>12</sup> Erman, *Ägyptische Grammatik* (3d ed., 1911), p. 102.

<sup>13</sup> Wilson, "The Eperu of the Egyptian Inscriptions," *AJSL*, XLIX (1932/33), 275 f.

<sup>14</sup> *BASOR*, LXXVII, 32 f. Cf. *Archaeology of Palestine and the Bible* (1932), pp. 206 f.

*sepher* and *kopher*. If this is true, then the assumption of such a change remains a highly precarious theory. The burden of proof for the actuality of the occurrence of the transition in this word lies on those who advocate the identification of *Hapiru* and Hebrews; the purely theoretical possibility that such a change might have taken place can carry only slight weight over against the solid fact that the two forms must be considered different until proven identical.

Here a point made by Rowley<sup>15</sup> might well claim attention. He asserts that "the view that the *Habiru* or *Hapiru* of the Amarna letters represent one of the waves of Hebrew or Israelite immigrants does not rest merely on this [philological] equation, nor would it fall if the equation were definitely ruled out." The only other thing beside etymology that can underlie the conviction of a relationship between the Hebrews and the *Ha-bi-ru* is the assumption that the latter are foreign invaders who are conquering the country after the manner of the Israelitic tribes of the Book of Joshua. But that assumption is itself very doubtful in the light of the new information concerning the *Ha-bi-ru*.<sup>16</sup> It seems to us far more likely that we are concerned with social revolution within the land than with invasion from without. Such revolutions took place repeatedly in Egypt and elsewhere. Why not in Palestine? If we find the statement in Amarna 67:17 that "Aziru is an escaped dog like the *sa.GAZ*" this seems to us to suggest quite clearly that the *sa.GAZ* and *Ha-bi-ru* in the Amarna period are runaway or emancipated servants (many of them doubtless men who were in military service) who band together and seize control of local towns and communities.<sup>17</sup>

But whether this view is correct or not, our intention in presenting it is merely to show that the other main prop on which the identification of *Ha-bi-ru* and Hebrews rests involves an "assumption" that itself now requires further proof. It is not our intention here to enter into that *Ha-bi-ru* problem in its full range. It is a problem in the oriental history of the second millennium B.C. and will have to be settled in the light of inscriptional and archeological data. The Israelitic sources cannot be used as "historical" source material of the first

<sup>15</sup> *Op. cit.*, p. 93.

<sup>16</sup> Lewy (*op. cit.*, XIV [1939], 615) says "no passage in the Amarna letters shows that they are nomadic invaders."

<sup>17</sup> Cf. now Lewy, *ibid.*, pp. 616 f.

rank in dealing with such a problem, any more than Herodotus carries the weight for Assyro-Babylonian history that a cuneiform text carries. The problem of the name "Hebrews" is, in our opinion (as will be set forth below), a problem of the early part of the *first* millennium and for the present at least, or until more inscriptions having bearing on this age come to light, will have to be investigated as a biblical problem. When the proper limitations are not observed, only confusion results, and the entire *Ha-bi-ru*—Hebrew discussion has been tinged with that sort of thing. In the following pages we shall investigate the name "Hebrews" exclusively on the basis of the Old Testament. This is the more necessary since the only recent study, that of Parzen,<sup>18</sup> reaches what we believe to be erroneous conclusions. We are particularly concerned with one point which everybody seems to take for granted, viz., that the name "Hebrews" is old and was applied to the Israelites from the very beginning of their history. This is an assumption which certainly ought to be tested.

The chief proof for the supposition that the name is ancient is its use in the narratives concerning Israel's beginnings. It is rather naïve, however, to suppose that this usage accurately reflects that of the times concerning which these narratives speak. In the light of our present insight, the nation called Israel crystallized from various groups which came to unite or were united as a result of forces or circumstances that need not be further discussed in this place. All evidence points to the fact that "Israel" was the common name for this union of tribes from the very beginning. There is no evidence and very little likelihood that the separate groups previously had a common name for themselves or were designated by such a name, except this usage of sources many centuries younger than the events they purport to relate. The only actual fact at our command is this—that documents dating from the period of the divided monarchy, presumably the ninth and eighth centuries, employ the term "Hebrew" or "Hebrews" in a few connections as a designation for an Israelite or the Israelites. What occasions surprise is not so much the existence of an alternate name as the spotty nature of its occurrence. We must make clear to ourselves that this name plays no discernible role in the historical life of the people. Nowhere in Joshua, Judges, II Samuel,

<sup>18</sup> Parzen, "The Problem of the Ibrim in the Bible," *AJSL*, XLIX (1933), 258 f.

I or II Kings, or in the great stream of prophetic literature (cf. below on the seeming exception in Jeremiah) is this name employed. Its actual use is restricted to a few clusters of passages (where it occurs repeatedly) and to several isolated instances. A little reflection will show that under these circumstances the term cannot be regarded as very deeply rooted in the national tradition.

We must first of all glance at the tradition itself to see if we can find any principle or set of principles governing the choice of the word where it is used. The first "cluster" of passages is that found in the Joseph stories, which dwell on Egyptian-Israelite relations. Joseph is referred to by the Egyptians as a "Hebrew man," "Hebrew slave," "Hebrew boy, slave of N. N." (Gen. 39:14, 17; 41:12). Joseph speaks of Palestine as the "land of the Hebrews" (40:15), and the narrator himself contrasts Egyptians and Hebrews on a point of custom (43:32). The second "cluster" of passages is found in the Moses cycle, which also dwells on Egyptian-Israelite relations. We hear of "Hebrew midwives" and of "Hebrew women," who in one case are contrasted with Egyptian women (1:16, 17, 19; 2:7); and of Hebrews in general (2:6). In all these instances the word is used either by Egyptians in speaking to or about Israelites or by Israelites in speaking of themselves to Egyptians. This is true also in the case of all the occurrences of the plural masculine except two. In 2:6 we hear of Hebrews in the mouth of the Egyptians and in 3:18, 5:3, 7:16, and 9:3 of Hebrews in the mouth of Israelites. But it is to be noted in 2:11 and 13 that the narrator himself uses the word in speaking of his own people.

The third "cluster" of occurrences is composed of passages which are found in I Samuel. They are concerned with Philistine-Hebrew relations. We hear the Philistines speaking of the Israelites as Hebrews in 4:6 and 9, 13:19, 14:11, and 29:3. In 13:3 Saul is supposed to have said, "Let the Hebrews hear," but we share the opinion of many critics that this verse must be rearranged and that it originally spoke of the Philistines hearing that the Hebrews had rebelled (cf. the commentaries and Kittel's *Biblia Hebraica*), so that the word was used by the narrator. In 13:7 the word "Hebrews" must probably be gotten rid of entirely; the subject of the verb "and they crossed over" (emend to impf. cons.), is still the men of Israel in verse 6. In

14:21, finally, we have the word used in the mouth of the narrator. Thus the same mixed situation occurs here as in the Joseph and Moses cycles.

There is also a fourth group of occurrences, which, though not in proximity, form a cluster from a material point of view—Exod. 21:2, Deut. 15:12, and Jer. 34:9 and 14. It is obvious that Jer. 34:9 and 14 are closely related to Deut. 15:12 and do not have any independent value. But Deut. 15:12 is merely a restatement of the law of Exod. 21:2 for a different age. Except as attestations of the continued existence of the word "Hebrews," the secondary passages are without significance for the purposes of this inquiry. Exod. 21:2 is their fountainhead and need alone concern us.

Finally, there are the two isolated occurrences in Gen. 14:13 and Jonah 1:9. The former would, of course, be of great importance if the document were an ancient one. Since we cannot accept that view and see no strong arguments lending support to it, we feel that the reference to "Abram the Hebrew" carries no historical weight. The Book of Jonah being admittedly late, its allusion to the hero as a Hebrew does not lend much light to the subject.<sup>19</sup>

The first question awakened in the mind as one scans this material is this: Is the word "Hebrew" an ethnic term?

Under the influence of the Ha-bi-ru-Hebrew equation and in the light of the new materials given to the world by Chiera it became clear in recent years that Ha-bi-ru could not be ethnic; therefore, there has been a tendency to read this result also into the Old Testament use of "Hebrew." The strongest argument in this direction was put forward by Alt, who claims that in Exod. 21:2 *Ibri* signifies a debt slave (cf. below).<sup>20</sup> But we doubt the probability of that exegesis, for would not *ebed* be unnecessary and tautological if *Ibri* per se meant debt slave? Surely the idea would not have occurred to Alt if it had not been for the texts from Nuzi. Meek, because of the same

<sup>19</sup> We will accept as correct the statement of Parzen that the name is not used extensively in post-biblical sources, the exceptions being Judith and II Maccabees. In the former we have mere imitation of biblical usage; in the latter, Alexandrian atmosphere, where the term "Jew" aroused hatred and contempt, so that "Hebrew" was artificially revived. It is interesting to note that Paul calls himself a Hebrew with obvious pride (II Cor. 11:22; Phil. 3:5).

<sup>20</sup> Alt, *Ursprünge des israelitischen Rechts* (1934), pp. 19 f. Cf. also Gordon, *Biblical Archaeologist*, III (1940), 12.

general line of thought, would even deny the ethnic use in Gen. 14:13 and finds justification for this in the fact that the LXX renders περάτης.<sup>21</sup> But, as Noth rightly says, this LXX translation proves nothing except the understandable need felt by the Greek translator to make clear what was thought to be the significance of this designation at the first place in which it occurs.<sup>22</sup> Since *Ibrī* is nowhere used as a word for a transient or nomad, we have scant grounds to regard it as an appellative of that meaning in Gen. 14:13.<sup>23</sup> If the nonethnic rendering cannot be defended in these two places, it is impossible to prove it anywhere else. Curiously enough, some of the scholars who start out with the claim that "Hebrew" is originally nonethnic of necessity end up by theorizing as to how it became ethnic in time (e.g., Speiser and Lewy). Noth, however, alluding to the inconsistency of such a procedure, frankly admits<sup>24</sup> that, if *Ibrī* really were an ethnic designation, one would have to separate *Ha-bi-ru* and *Eperu* from it entirely.

We must now ask: Why did the writers who employ the term "Hebrew" choose to use it when they did? Since *Ibrī* pl. *Ibrīm* < *Ibrīyyīm* is a gentilic form,<sup>25</sup> Landsberger has suggested that it serves as a gentilic substitute for "Israelite."<sup>26</sup> For some reason not quite clear a *nisbe* of "Israel" is found only in II Sam. 17:25 where, however, it is an error for "Ishmaelite" (cf. I Chron. 2:17) and in the late passage Lev. 24:10 f. (cf. also the N.T. John 1:47, etc.). In our older texts, Alt asserts "Israelite" is always expressed by the phrase

<sup>21</sup> *Op. cit.*, p. 7.

<sup>22</sup> Noth, "Erwägungen zur Hebräerfrage," *Festschrift Otto Procksch* (1934), p. 107.

<sup>23</sup> The LXX translator seems to have etymologized the term as "wanderer." The word appears again only in Philo i. 349. Parzen suggests that the translator probably read *ha-ōbēr* in Genesis, chap. 14, but that can hardly be taken seriously.

<sup>24</sup> *Op. cit.*, p. 101. Noth's theory is that *Ha-bi-ru*—Hebrew is a self-designation of the nomads who have entered a settled region and tent there without property rights. But this falls with the *Ha-bi-ru* equation and the disproof of their nomadism.

<sup>25</sup> This fact by itself should have discouraged comparison with the word *Ha-bi-ru*. The Nuzi fem. *Ha-pi-ra-tū* makes it especially clear, for the *nisbe* would be *hapiraitu* > *hapiritu*. The *nisbe* of the masculine *Hapiraiu*, as pointed out by Knudtzon (*op. cit.*, p. 47) occurs only in two isolated instances of the Cassite period, and there with the writing *b/pir* instead of the usual one. On these passages see Weidner, *Archiv für Orientforschung*, X (1937), 2 f.; Landsberger, *Archiv für Orientforschung*, X (1937), 140 f. Such isolated occurrence scarcely warrants the weight given it by Meek, *op. cit.*, p. 13. Böhl's attempt to link these instances with a "Hapirtu" connected with Elam (*Kanaander* . . . , p. 86) is no longer possible; the name is *Ha-tam-tu*. Cf. Poebel, *AJSL*, XLVIII (1931), 20 f.

<sup>26</sup> Landsberger, "Habiru und *Lulahhu*," *Kleinasiatische Forschungen*, I (1929), 329.

"Man of Israel" (Judg. 7:14, Num. 25:8 and 14; cf. also "a man from the house of Israel," Lev. 17:3, etc.).<sup>27</sup> Since *nisbe*'s are formed from similar names like Ishmael and Jerachmeel, one wonders why this situation should exist. Apparently foreigners did hesitate to form one, for Shalmaneser speaks of Ahab as "Sir'lai," using a *nisbe* form. One suspects editorial elimination of this form in the Old Testament literature for some reason that can only be guessed. But this much is clear—that, if "Hebrew" had actually served as gentilic for "Israel," we should find it used more consistently and not merely in several clusters of passages. It appears necessary, then, to seek for a different explanation.

It seems to us that where the name is employed it is an alternate name for Israel and is used purposely in place of the latter name. A certain nuance of feeling which the true national name conveyed to those who bore it was to be ruled out; or, conversely, a different nuance of feeling was to be expressed when the word "Hebrew" was introduced. It is wrong to go as far as Parzen does when he asserts that there was something derogatory in the term "Hebrew." But it is a fact that the term is chosen (when chosen at all) in situations where the Israelite is not a free citizen in a free community or on free soil. If one will assume for the moment that, in contrast to the rather exalted term "Israelite," "Hebrew" was more objective, one can readily understand why the Israelites in the house of slavery, Egypt, would be spoken of as Hebrews and, above all, why an Israelite man who became a slave would preferably be called a "Hebrew slave" in Exod. 21:2. This would also fit I Sam. 14:21, where the writer differentiates "the Hebrews who belonged to the Philistines" as vassals from "the people of Israel who were with Saul and Jonathan." We think it totally unnecessary to separate these Hebrews tribally from the Israelites, as is so often done. That the Philistines in all the other passages are made to speak or think of "Hebrews" rather than of "Israel" may well be due to the fact that the story-tellers consider them as subjects—now in a state of rebellion, it is true—but theoretically Philistine dependents. Even in Genesis, chapter 14, Abraham is a so-

<sup>27</sup> *Op. cit.*, p. 21. One wonders, however, whether the phrase does not have a slightly different nuance than the use of the gentilic would convey—referring not so much to derivation as to present membership in the national religious community?

journer who pays tribute to Melchizedek, and in Jonah the prophet is a helpless man, away from his land and his people.

Having found at least a provisory explanation for the use of the name where it is used, we must now attempt to approach the matter of its origin. Was it a designation which outsiders actually used of Israel or is it a self-designation? The Egyptians had their own terms for the Asiatics of Palestine, and if one rejects the link with the Eperu-*Hapiru*, Hebrew is not found among them. "Israel" is directly mentioned in the stele of Merneptah. Mesha of Moab speaks of Israel, evidently meaning the northern kingdom, and also refers to one tribe of the Israelites as "men of Gad" (גָּדִים). This suggests that the Philistines, too, were wont to use the national name or the names of specific tribes with whom they came into contact. It seems to us, therefore, that "Hebrew" is purely a self-designation. But while this conclusion, if correct, relieves us of the expectation to find the "Hebrews" referred to in foreign inscriptions, it still leaves the origin of the term in obscurity.

To account for the name, it might be well to take into consideration the possibility that it is the name of an early group which was absorbed by the Israelites. We today speak of Britons and Anglo-Saxons as though they were alternate names, whereas historically they represent totally different peoples. Might not the alternate name "Hebrews" also be the real name of an earlier stock? The possibility cannot be ruled out, but it seems strange that there is no mention of a tribe of "Hebrews" in our earliest documents, even though they reflect such matters as the existence of a tribe of Makir and the early importance and subsequent decline of such tribes as Reuben and Simeon. It would seem, therefore, that there was no actual Israelite group with the tribal name of Hebrews involved in the early settlement of Canaan.

Another possibility seems more attractive, viz., that we here have a designation that came into vogue some time in the early history of the monarchy as a result of Israelite self-orientation in the world in which it had become a power. The question who are we and who are our next of kin must inevitably arise in youthful peoples that come to have a national consciousness. One answer that, as we shall see, impressed itself enough to gain widespread acceptance at a certain time

was this: we are *Ibrim*. That per se has a touch of racialism in it and implies separation from certain near-by elements and a linking-up with other more remote elements. For the antithesis to the near-by elements (Canaanites, Philistines) is a vital part of the national consciousness.

At this juncture one might well argue that "Hebrews" was the name of a distant group, contemporary or well remembered from former days, like the Aramaean *Aḥlamē* of whom we hear so much in the Assyrian inscriptions. Tradition (or speculation) asserted: we went forth from them. At a much later time, one might continue, it was no longer so clear who the *Ibrim* were, and the deuteronomistic passage (26:5) gives a modernized answer to the same question of origin and kinship when it says "a perishing Aramaean was my father."

It can hardly be doubted that both Eber > *Ibr* and *Ibrim* are possible tribal names. J. J. Hess has adduced some close parallels to both when he points out<sup>28</sup> that it is said in the Tāj el 'Arus, "And *al Ibr* is a tribe that stretches along the Euphrates to the Syro-Arabian desert, and which As-Şagāni mentions, and *Banu ɻ Ibr* is (also) a tribe but another than the aforementioned." He also cites from E. C. Ross's list of the Beduin tribes of Oman one that is identical with *Ibrī*=*Ibrim*: "Name of tribe, adjective form: *Ibrī*. Collective plural form *el Ibrī-in*" and says that "tradition of Oman asserts that these tribes came from the west, and may once have dwelt along the Euphrates." The latter point would seem to imply reflection on the etymology of the name, as having something to do with the shore of a river. It should not be overlooked, however, in the case of the names *al Ibr* and *Banu ɻ Ibr* that the use of the article betrays consciousness of the appellative character of the name, whereas it is not clear that biblical Eber was anything more than a name.

But, unfortunately, we are unable to prove the existence of a contemporary tribe of Eber or *Ibrim*: hence it seems advisable to look for a different solution. Now the explanation of national relationships in genealogical form was an especially easy and vivid way of dealing with the need of self-orientation in the world. Not only the Hebrews but even the Greeks followed much the same plan.<sup>29</sup> Various construc-

<sup>28</sup> J. J. Hess, "Beduinisches zum Alten und Neuen Testament," *Zeitschrift für die alt-testamentliche Wissenschaft*, XXXIII (1915), 120 f.

<sup>29</sup> Cf. Meyer, *Geschichte des Altertums*, III (2d ed., 1937), 290.

tions arose at different times. The theory that there was an ancestor Eber may be one of these.

Our Book of Genesis gives an interesting glimpse of this rivalry of theories which resulted eventually in harmonistic compromises. Thus in the early poetic lines of Gen. 9:25–27 Shem stands as Israel's ancestor and representative, with no thought of larger horizons; the outlook is purely Palestinian—Shem (=Israel), Canaan, and Japheth (Philistines, Thekel, etc.) are three groups descended from the aboriginal vintager Noah. In the later Jahwistic stratum where the vintager Noah has been identified with the Babylonian flood hero of the Gilgamesh epic, Shem is no longer identified with Israel but moves up into a more remote position as ancestor of a group of peoples. Now in J's system we find Eber referred to in a manner showing that he has been demoted: "To Shem, moreover, the father of all the sons of Eber was born (offspring)" (10:21). The artificiality of this statement is patent; the father of all the sons of Eber can only be Eber. Shem has crowded him into the background owing to the larger panorama the author-editor desired to obtain by connecting up with the (newly elevated) flood hero and his sons. P in his re-working of J's system makes Eber the son of Arpachshad whom J had put in the group Elam, Ashur, Arpachshad, Lud, and Aram, and provides for a direct line of descent leading from Shem through Arpachshad and Eber to the ancestors of Israel. But perhaps Shem and Eber were not the only rival ancestors. In his mention of Peleg, J says that under him "all the earth was divided" (vs. 25); unless it is a secondary element suggested by the desire of punning on the name, this statement alludes to a system in which Peleg was the father of sons representing various groups of men. It is not clear, however, who the (two?) sons of Peleg were, though we can infer from P (11:18) that one was Reu. Peleg himself has now been demoted to the rank of one of two sons of Eber, the other being Joktan. In P's final system we hear of only one son of Eber, Peleg, and the latter is the father of Reu, from whom the line leads via Serug and Nahor to Terah. But another observation must be made. The system that put Eber at the head and made the sons of Eber ancestral to a line leading down to Israel, on the one hand, and to certain Arab tribes as far down as Hadramaut, on the other hand, is basically similar or even parallel to that which makes Abra-

ham father of both Isaac as ancestor of Israel and Ishmael as ancestor of an Arab group, or the other which makes Isaac father of both Jacob, rival ancestor of Israel, and Esau, ancestor of the Edomites. We thus get a glimpse of the fact that historical speculation, availing itself of the genealogical principle, attempted to establish a considerable variety of systems which eventually resulted in certain compromises.

The possibility that the name "Eber" is secondarily personalized from a geographical name is one that now needs to be investigated. This is particularly recommended by the fact that so many other geographical names are found in genealogy. In J's list of Shem's sons we doubtless have geographical names, though Arpachshad is still obscure.<sup>30</sup> P makes Eber the son of this Arpachshad and the progenitor of the line Reu, Serug, Nahor, Terah. Of these names, Serug and Nahor are certainly personalized geographical names.<sup>31</sup> I once explained Terah as derived from the city name *Til-ša-Turahī*, likewise in the vicinity of Harran, and still think that, owing to the geographical "environment," this is not entirely ruled out,<sup>32</sup> but the discovery of a personage named *Trḥ* in an obviously lunar role in the Ras Shamra texts<sup>33</sup> and the vocalization of the biblical Terah like *jérab*, "month," as well as the appropriateness of the idea that the moon-god should live at Harran (Gen. 11:32) makes it more likely that the name is to be explained from that quarter.<sup>34</sup> An exception, however, only helps to prove the rule. In view of the large use made in this list of personalized geographical names, and also in view of the fact that the most natural etymology strongly suggests some connection with the other shore of a river,<sup>35</sup> we have very good cause to look for the origin of the ancestor Eber in a geographical term of some sort.

<sup>30</sup> This name is particularly vexing. I expect to return to it in another connection. Meanwhile cf. my *Aram and Israel*, p. 18.

<sup>31</sup> Cf. Schrader, *Keilschriften und das Alte Testament*<sup>2</sup>, pp. 477 f.; my *Aram and Israel*, pp. 18, 24.

<sup>32</sup> ZAW, XL (1922), 153 f.

<sup>33</sup> Dussaud, *Les convertes de Ras Shamra et l'ancien testament* (1937), p. 81, n. 4, and p. 102, n. 3.

<sup>34</sup> The "lunar" interpretation of the figure Terah, hinted at by Jeremias, *Das Alte Testament im Lichte des Alten Orient*<sup>2</sup>, p. 259, thus has received unexpected confirmation.

<sup>35</sup> Another possible etymology is "one who has passed over." Cf. Aešcoly, "Falasha-Ibrim," *AJSL*, LI (1935), 127 f., for a parallel. But in that case one would have to assume the name to be a designation given by others and not a self-designation. The analogy quoted above from the tribes of *al 'Ibr*, etc., points more in the direction of connection with the shore of the river. Cf. my *Aram and Israel*, pp. 31 f.

A clue to such a geographical term seems to be given to us by the passage from the Balaam oracles referring to Eber (Num. 24:24). Now it is quite true, as Dillmann first pointed out, that Num. 24:20–24 do not seem integral in the connection,<sup>36</sup> but this does not prove that the words are necessarily a very late fabrication; it merely suggests that they are of separate origin. It is difficult to see what interest late times could have in Amalek or Cain or an otherwise unknown Eber. The oracle predicts the end of Amalek, considered a highly important nation, and the deportation of Cain by Ashur in spite of its impregnable position on a high rocky plateau, and then closes with the words

And ships (will go forth) from the side of the Kittaeans  
And will humble Ashur and will humble Eber  
And moreover it (?) perishes for aye.

Mention of the Kittaeans has suggested to some scholars that this is an interpolation from the Greek period because "the land of Chittim" in I Macc. 1:1 is Greece, whence Alexander went forth.<sup>37</sup> Meyer rejects that idea entirely because of the presence of the verses in the Samaritan Pentateuch.<sup>38</sup> I am not sure that this particular argument is valid, for it might conceivably be shown that the Samaritanus was re-edited in the light of the text set up as authoritative by the Jews. But allusion to the isles of the Kittaeans in Jer. 2:10 certainly shows that the term could be used in pre-Exilic times. Furthermore, I can see no connection between Num. 24:24 and the history of Alexander; the latter merely used his fleet to ferry his army across the Dardanelles and then marched overland to his wars with the Persians, for whom Ashur and Eber would have to be a sort of cryptic designation. Num. 24:24 presupposes a situation in which ships come, apparently via Cyprus (to which the name Kittaeans, derived from Kition, primarily belongs), and land troops that attack Ashur and Eber. Dan. 11:30 sees the prophecy fulfilled in the coming of the Romans at the time of Antiochus Epiphanes, but that very fact seems to us to prove that Num. 24:24 is much older. We must reject as unthinkable the idea that it is so late an interpolation.

<sup>36</sup> Cf., in general, Mowinckel, "Der Ursprung der Bilaamsage," *ZAW*, XLVIII (1930), 233 f. Eissfeldt, "Die Komposition der Bilaamerzählung," *ZAW*, LVII (1939), 212 f.

<sup>37</sup> Cf. Baentsch, *Handkommentar: Exodus-Leviticus-Numeri*, ad. loc.

<sup>38</sup> Meyer, *Die Israeliten und Ihre Nachbarstämme* (1906), p. 321.

Before proceeding further a word must be said about the method of interpreting this prophecy. Dare we use in this instance the principle of the post-eventum prediction? Meyer doubts the necessity of always historically interpreting words of a soothsayer, especially words that may be intentionally obscure.<sup>39</sup> He asserts that some of these words will never be satisfactorily interpreted. But since the post-eventum principle regularly gives us the key elsewhere in this type of prophetic passage, one is reluctant to back down in this instance without trying to make use of it. If one employs it, it would seem to necessitate our assuming (1) that the Assyrians carried off the Kenites; (2) that "Eber" and Ashur were humbled by troops landed by ships from the west. We would then have allusions to historical events not otherwise known. The matter leaves the historian somewhat helpless. Why should the Assyrians have troubled themselves to deport the Kenites and when could they have done it? How was it possible for ships from Kittim to humble Ashur and Eber? The classical philologist, Dornseiff, takes the phrase "from the side of Kittim" literally and says this means ships of Kition, the Phoenician city on Cyprus.<sup>40</sup> He interprets this with the help of a list of the sea rulers from the fall of Troy to 480 B.C., which a historian of the time of Caesar, Castor of Rhodes, put together and which reports a sea rule of the Cypriots 880–846 B.C. But unfortunately this period coincides with an era of great Assyrian power under Ashurnasirpal and Shalmaneser, so that an actual humiliation of Ashur by invaders from the sea seems out of the question. Furthermore, the great importance of Tyre at this period precludes a Cyprian sea rule. Meyer is doubtless right in dismissing the list of Castor of Rhodes as a worthless fabrication.<sup>41</sup>

Under the circumstances, then, it seems as though we have actual prophecy in Num. 24:24 spoken at a time when Assyria had become known as a factor of history for the West and when Amalekites and Kenites were still enjoying an independent existence and seemed formidable to Israel.<sup>42</sup> The information that we have concerning both of these tribes is rather meager, but we may hazard the guess on the basis

<sup>39</sup> *Ibid.*

<sup>40</sup> Dornseiff, "Antikes zum Alten Testament," *ZAW*, LV (1937), 135.

<sup>41</sup> Meyer, *Geschichte des Altertums*, II<sup>2</sup>, Part II (1931), 62.

<sup>42</sup> Cf. Meyer, *Israeliten*, p. 392. He, too, considers the passage ancient.

of what we know that the situation fits the early period of David's rule, prior to his subjection of Edom. The name "Eber" can hardly have referred to Israel, as Dornseiff assumes. Its proximity to Ashur in the prophecy and its place in the genealogies of Genesis, chapter 10, all point to Mesopotamia. It was apparently used at a certain time to describe the ancient Mitanni—territory which included the region that lay "across" the river (Euphrates)—the district around Harran to which Jahwistic tradition points as the home of Abram.

The establishing of a connection of "Hebrew" with a geographical "Eber" is ancient. It was already suggested by Aquila in his revised rendering of LXX in Genesis, chapter 14, as *περάτης*, a word used by Josephus *BJ* ii. 20. 4 for "one of the country across the water," i.e., Peraea. The narrow Palestinian viewpoint of the Roman period is not to be confused, however, with the broader international horizon of the Israelite era. In those days *hannahar*, "the river," was not the Jordan but the Euphrates. It is not likely that Eber had anything to do with Transjordania in Num. 24:24 or in the personalized ancestor of Genesis, chapter 10, for special district names prevailed for that area; a man from thence was a Gileadite, etc. Connection with the river Euphrates seems the only reasonable one, if a geographical link is to be sought.

Jensen recently raised the question of the connection of Eber with the term Eber-hannahar that we meet in Ezra 4:10 as the name for the province *west* of the Euphrates.<sup>43</sup> This is obviously a formulation from the viewpoint of the Assyrians or Babylonians (and Persians); for them Syria is Transeuphratensian. Jensen asks whether it is possible that the term "Hebrew" could have come up among the Jews in the Babylonian exile? We frankly think that it is impossible to make our references so late. But it is quite possible that prior to the rise of a province of that name, the people living in Syria or Palestine should have described the territory lying east of the Euphrates (from their point of view) as Eber-hannahar, and then abridged that term further to "Eber." That they used the phrase "other side of the river" in thinking about that region is obvious (Josh. 24:2, etc.), though we cannot prove that a name Eber-hannahar had an official

<sup>43</sup> Jensen, "Alttestamentlich Keilinschriftliches," *ZAW*, LII (1934), 123 f. Cf. also my *Aram and Israel*, p. 31.

status for that territory. But we think that Num. 24:24 shows that Eber was a familiar geographical term for it during a certain period—no doubt the very time when the vogue came up to describe the Israelites as "Hebrews." After that geographical term ceased to be used (presumably owing to the definite absorption of the territory by the Aramaeans which led to the use of the designation *Aram Naharaim* in Gen. 24:10 etc.), Eber became merely the name of an ancestor, as in Genesis, chapter 10.<sup>44</sup>

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<sup>44</sup> A word may be added here on the supplementary article of Lewy, "A New Parallel between Ḫabiru and Hebrews," *HUCA*, XV (1940), 47 f. In his note on the Ugaritic spelling of the word he takes in effect the same line as Rowley and Jack. He seeks to bolster the reading *Ha-bi-ru* by referring to the god Ḫa-bi-ru at Ashur (Schroeder, *Keilschrifttexte aus Assur verschiedenem Inhalts*, Pl. 37, col. II, l. 9; cf. Gustav's *ZAW*, XL [1922], 313 f.). But the spelling may be traditional and the god imported (cf. Albright, *BASOR*, LXXXI [1941], 20). It is also possible, however, that the name of the god is of an entirely different derivation. Even the god Amurru is not necessarily to be associated with the Amorites (cf. *Reallexikon für Assyriologie*, I, 99). And in the case of the Ḫa-bi-ru we are dealing with a social class rather than with an ethnic group, making it even more difficult to believe that it could lend its name to a divinity! If Lewy says that I overlook the fact that the discussion of the Ḫa-bi-ru-Hebrew problem no longer centers about a chiefly linguistic question but rather on the Nuzi analogies, he will find that the above article takes the basis from underneath his own discussion. Proof of the survival of Hurrian or any other line of law or practice in the Palestine of the first millennium is interesting and valuable but can show nothing as to the origin of the Israelites.

## THE DATE OF THE BYBLOS TEMPLES

### BUILDINGS II, XVIII, AND XL<sup>1</sup>

ROBERT J. BRAIDWOOD

In his recent *Fouilles de Byblos* (Paris, 1939), Volume I, Maurice Dunand publishes three buildings of monumental character and the evidence for his interpretation of their dates. The buildings in question were set on sloping ground, and their excavation, in view of the differential levels involved, made for exceedingly complicated architectural digging. The success which M. Dunand achieved in the excavation and presentation of these buildings deserves only the highest praise. The matter in hand is merely concerned with his interpretation of the evidence for dating these buildings. Were it not for the specific invitation made to the reader of his volume to make original interpretations, I should hesitate to present mine publicly, for I am firmly convinced that no opinion can bear so much weight as that of the excavator himself.

Fully realizing the huge responsibility with which the excavations of Byblos charged him, M. Dunand dug entirely by a system of 0.20 m. *levées*, each *levée* peeled off one after the other, and each kept "rigoureusement horizontale." The system has both advantages and disadvantages. It is only pertinent here that objects are generally noted by *levée* rather than by relation to a specific occupational floor. The plan presented here (Fig. 1) was made by assembling on one sheet the tracings of the individual buildings published in the *Fouilles de Byblos*, Volume I. The section was constructed on the basis of the assembled plan and with the aid of various statements in the text (pp. 290-308).

The buildings and their dates as published by M. Dunand are:

1a. Building II (*dernier état du corps principal*)—Middle Kingdom, on the basis of the Twelfth Dynasty materials found in the jars of foundation offerings, underneath pavements of the building.

1b. Building II (*premier état*)—Old Kingdom to as early as the offerings of Khasekhemui (pp. 298, 304).<sup>2</sup>

<sup>1</sup> I am obliged to George R. Hughes and Richard A. Parker for checking the pertinent Egyptian inscriptions, and to Harold D. Hill for the finished drawing.

<sup>2</sup> Since the Khasekhemui inscription was found on the surface, the dating is by implication only and need not apply specifically to Building II.

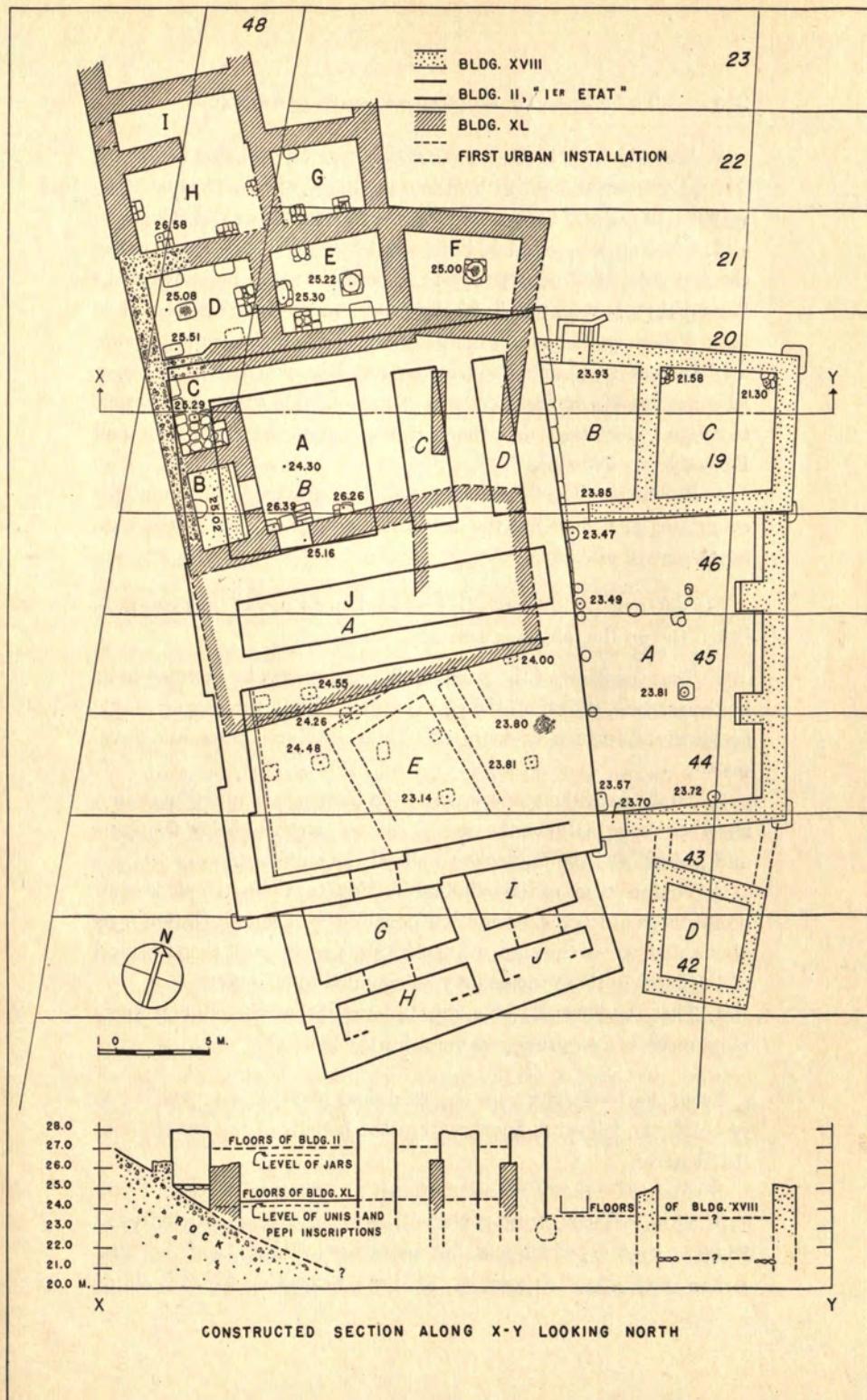


FIG. 1.—Plans of the Byblos Buildings Assembled and Superimposed, and a Constructed Section

2. Building XVIII—built against Building II, probably before the Third Dynasty (p. 304), with various additions, of which a great staircase is said to have been added toward the end of the Old Kingdom.

3. Building XL—overlaid in fair part by Building II; this is called the first important construction to be built above the buildings of the “first Urban Installation.” M. Dunand says of Building XL: “Elle était déjà en ruine quand furent construits les plus anciennes parties du Bâtiment II, temple que les pharaons Khasekhemoui, Chéops, et leurs successeurs dotèrent de leurs offrandes.” He makes no attempt to assign a more exact date than this implication that it is pre-Second Dynasty (pp. 295–96).

4. Buildings of the “Première installation urbaine,” which underlie everything at Byblos but the aeneolithic necropolis. No precise date is attempted, of course.

My difference in interpretation is based on a study of the plans published and on the following assumptions:

1. That the floors of the buildings in question can be assumed to lie at those levels, shown on the plans, which contain such occupational architectural features as doors, sills, sockets, column bases, and pavements.

2. That where the stone walls of the buildings in question do not show the above features, the excavation has proceeded below the floors and has exposed the *foundation* walls of the buildings.

3. That in rooms of the buildings in question, where the plans show no intrusion of later walls or other possibilities of contamination from above, and, where the occupational features are present, then the level of the floors in those rooms may be assumed to be intact.

4. That the latest datable objects found below these intact floors must serve as a *terminus post quem* for the building.

From the levels given on the published plans, it was possible to establish the following floors, within the bounds of the assumptions listed above.

Building II—shows no occupational architectural features below  $\pm 27.50$  m. contour (from sea-level), save for a patch of cinders at 23.80 m. contour, which must belong rather to the walls of the “first Urban Installation” exposed at that level, below Court E of Building

II. The foundations of Building II descend as low as 22.60 m. contour, and the north run of the main east wall of the building rests on a wall of much larger stones which bears slightly eastward and is overlaid by the wall of Building XVIII, Room B, northwest corner, at least below 23.93 m. The change in masonry noted in *Fouilles de Byblos*, Volume I (i.e., Pl. XVI), as well as the cross-walls which exist within Room E just under 25.10 m. (Pl. CCVII), are strong arguments for M. Dunand’s division of Building II into two phases, but *no* occupational features or floors below those at  $\pm 27.50$  m. are evidenced in the plans. One more point which assures the height of the floor of Building II in its northernmost portion is the fact that the walls of Building XL are preserved as high as 26.67 m. (p. 297).

Building XVIII—shows a number of doors, sockets, pillar supports(?) at  $\pm 23.75$  m. contour, but also two clumps of stones in the corner of Room C at  $\pm 21.40$  m. The preponderant number of occupational features at the higher level, however, makes the assumption of the floor at  $\pm 23.75$  m. more likely. The fact that the floors of Building XVIII are so much lower than those of the associated Building II must be accounted for by the general west-east pitch of the mound at this point. Even some clearance of older debris from along the east side of Building II may have been made in preparation for Building XVIII. Whatever the reason for the depth of the floors of Building XVIII (with levels comparable to those of the “first Urban Installation” just west of it under Court E, Building II), there can be little doubt that Building XVIII was annexed to Building II, as Dunand proposes, especially since its walls so plainly cover the basal wall of Building II, in Building XVIII, Room B.

Building XL—shows a considerable number of occupational features, in fact, it is practically possible to establish its floors room by room, between  $\pm 25.25$  m. and 24.30 m. contours. The difference in floor level is probably due to the proximity of rock below (the western part of Building XL is cut out of rock). How deeply Building XL is founded otherwise is not published, but its walls were preserved above floor level to as high as 26.67 m. contour.

With these floors once set out as indicated above, it is merely necessary to go through the inventory of objects in the *Fouilles de Byblos* to discover which are the latest pieces to underlie the floors of the various

buildings. These, especially in Building XL, have been chosen from areas where there is no published evidence to make one expect intrusion. The dating pieces are:

Building II—the Twelfth Dynasty jar deposits, said to have been buried under the pavement in Court E, 27.40–26.80 m. contours.

Building XVIII—fragments of inscriptions: No. 5136, with Old Kingdom titles similar to those of the Hesire panels of the Third Dynasty, in Room A, 23.0–22.8 m.; No. 5141, either Pepi, Room F (cf. Pl. CCVII), 23.0–22.8 m.; No. 5191, Pepi I, Room E, 22.8–22.6 m.

Building XL—fragments with cartouches: No. 3860, a Pepi, rectangle 48 ("Salle B"), 24.4–24.2 m.; No. 3980, Unis, rectangle 48 ("trouvé vers le centre du rectangle," i.e., must be under floors of Building XL, Rooms D, H, or I), 24.2–24.0 m.; No. 3981, Unis, rectangle 48 ("trouvé à côté du no. précédent"), 24.2–24.0 m.

On the basis of the evidence given above, my interpretation of the dating of Buildings II, XVIII, and XL would differ from the interpretation published by M. Dunand as follows:

1. Building II was built some time after Building XL (V–VI dynasties), which underlies it, probably quite close to the date of the Twelfth Dynasty jar deposits buried under its pavements. Whether there was an earlier stage of Building II or not, there is certainly no earlier fixed dating evidence published for it.

2. Building XVIII can be no earlier than the two Pepi inscriptions. On the other hand, if it must be considered an annex of Building II (for which the architectural evidence is excellent), then we must assume that there was no contamination of the earlier levels with contemporary materials when it was founded.

3. Building XL can be no earlier than the Fifth-Sixth Dynasty inscriptions found below its floors. Its latest limit is, of course, fixed by Building II above it.

In conclusion, the reader must again be reminded that this interpretation is based on such knowledge as can be gleaned from the published report. M. Dunand had the very evidence in his hands, hence his interpretation must be given first consideration.

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UNIVERSITY OF CHICAGO

## WOMEN AND THE STATE ON THE EVE OF ISLAM

NABIA ABBOTT

We have seen in a preceding article\* that ancient Arabia had its queens and that some of these were also the priestesses of their local gods. They thus paralleled the priest-kings so familiar in the history of the entire ancient Orient. We have also seen that after the astonishing careers of the Emesan Julias and the Palmyrene Zenobia, Arab queens receded more or less into the background. Thus, for the last few centuries of the pre-Islamic period, characterized by the pious and proud Moslems as *al-Jāhiliyah*, or the "Age of Ignorance," we can point at best to a shadowy Ḥimyārite Balqis, a half-forgotten Ghassānid(?) Mawia, and a humiliated and bereaved Lakhmid Hind. The great majority of the royal women of these dynasties and of that of Kindah figure little or not at all in the available records. This may be due partly, as already pointed out, to the paucity and poverty of these records, if not indeed to the prejudice of the second- and third-century Moslem recorders. On the other hand, the situation may be reflecting some loss in public position suffered by the women in the centuries immediately preceding Islam. Changing social conditions, due in part to contacts with neighboring peoples and kingdoms, may have deprived the Arab woman of this period of some of the public prestige and privileges enjoyed by her earlier sisters.

However, it must not be inferred that the influence of the Arab woman had become negligible in the various phases of both private and public life. In her home the free Arab woman of all classes in her time-honored role of legal wife and mother expressed herself freely and forcefully. In poetry, the major literary passion of pre-Islamic Arabia, the Arab woman figured large. Not only did the romantic poets sing her praises in passionate verse but the chivalrous Arab, as yet not too civilized, coveted and prized her opinion as literary critic. The story is told of how the Kindite "vagabond prince" and greatest of Arab poets, Imrū al-Qais, during his wanderings settled for a while

\* See "Pre-Islamic Arab-Queens." *AJSL*, LVIII (1941), 1–22.

among the Banū Tayy and married one of their women known as Umm Jundab. One day Imrū and that other famous poet, 'Alqamah ibn 'Ubaidah, fell to arguing about their respective merits as poets, and neither would give preference to the other. Finally 'Alqamah suggested that Imrū's wife should decide the question, and to this Imrū readily agreed. Umm Jundab then called on each to compose, in the same meter, a poem on the qualities of the horse, and, when this was done, she decided in favor of 'Alqamah. Imrū, annoyed at the decision, divorced his wife whom 'Alqamah then married.<sup>1</sup> Though this particular story may well be legendary, since the rivalry between the two famous poets has been questioned,<sup>2</sup> the presence of a woman literary critic in a land and at a time when poetesses received full recognition according to their merits is not at all improbable. For the pre-Islamic Arab accepted, as a matter of fact, woman's poetic contributions for general circulation.<sup>3</sup> In this connection one need only mention that greatest of a long line of pre-Islamic Arab poetesses, Tumadīr al-Khansā, who witnessed the advent of Islam and accepted the new faith, and whose poetry won the approval and praise of Mohammed.<sup>4</sup>

In the religious life, which generally linked up with the economic and political development and welfare of the people, a certain class of women played definite and well-recognized roles. Among them were the *kāhinah* or woman-seer and soothsayer, the *rabbat al-bait* or temple priestess, and, now and again, the more pretentious *nabīyah* or prophetess. They seem to have exercised their functions in the same way as did their masculine counterparts, the *kāhin*, *rabb al-bait*, and *nabī*. The known references to the *kāhinah* in Arabic literature are too numerous to list.<sup>5</sup> The traditions associate one of these women with almost every major move of tribal policy or migration. There is, for instance, the well-known story of al-Zarqā, whose visions and pronouncements guided the movements of the Yamanite Tanūkhids

<sup>1</sup> *Aghāni*, VII, 126-28.

<sup>2</sup> Cf. Clément Huart, *A History of Arabic Literature* (New York, 1903), p. 16.

<sup>3</sup> Lewis Cheikho, *Riyāḍ al-Adab fī Maṭāḥī Shawā'ir al-'Arab* (*Les Poëtesses Arabes*) (Beyrouth, 1897), a work devoted to pre-Islamic Arab poetesses.

<sup>4</sup> Ibn Ḥajar, *K. al-Isābah* (Calcutta, 1873), IV, 550; cf. Cheikho, *Commentaries sur le Diwan d'al-Hansā* (Beyrouth, 1896), pp. 19-23.

<sup>5</sup> *EI*, II, 624-26; Wellhausen, *Reste arabischen Heidentumes* (Berlin, 1887), p. 130; Anastase Marie de St. Elie, "La Femme du desert autrefois et aujourd'hui," in *Anthropos*, III (1908), 60; Lammens, *L'Arabie occidentale avant l'Hégire* (Beyrouth, 1928), Index, "Kāhinah"; Buhl, *Das Leben Muhammeds*, trans. Schaeder (Leipzig, 1930), p. 82.

northward to Ḥirah.<sup>6</sup> A similar and equally important role is assigned to the *kāhinah* Turaifah, who accompanied other Yamanite tribes into the Ḥijāz.<sup>7</sup> *Kāhinah*'s play their parts in the war of Basūs between Taglib and Bakr and in the Battle of Dhū Qār<sup>8</sup> between the Arabs and the Persians. A *kāhinah* consulted by the Quraish is said to have foretold Mohammed's prophetic mission by some twenty years.<sup>9</sup> Several of them are mentioned in Mohammed's time: There is the Quaraishite al-Ghaiṭalah,<sup>10</sup> there is the *kāhinah* of Banū Ḥadas,<sup>11</sup> there is Hudhaim the *kāhinah* of the Banū Sa'd, there is Fātimah bint al-Nu'mān at Makkah at the time of the Hijrah, and there is an unnamed *kāhinah* at Madinah of about the same time.<sup>12</sup> Neither did they quite disappear<sup>13</sup> with the coming of Mohammed, who was himself dubbed a *kāhin* by his opponents. The prophetess Sajāh, as we shall presently see, is likewise said to have started her role as a *kāhinah*.

The *kāhinah*, like the *kāhin*, was usually not restricted in action or movement. Not infrequently the *kāhin* performed also the duties of the *hākim*,<sup>14</sup> or judge-arbiter. Whether the *kākinah* as such performed this function is hard to trace, but the *hākimah*, or woman judge-arbiter, is met with in the traditions.<sup>15</sup> Sometimes the *kāhinah* became associated with the shrine of a specific goddess or temple and was then called *rabbat al-bait*, or the mistress of the temple. Just what her specific functions were does not seem to be clear, though their politico-religious nature is hardly to be doubted. She does not appear in the traditions as frequently as does the simple *kāhinah*; but this may be due to a change in her position in the few centuries before Islam. Or again it may be due to the change in meaning that the word *rabb*, and

<sup>6</sup> *EI* (*Suppl.*), art. "Tanūkh."

<sup>7</sup> *Aghāni*, XIII, 110; Mas'ūdī, *Murūj al-Dhahab*, ed. Meynard and Gourteil, III (Paris, 1864), 352, 379 ff.; for others see *ibid.*, pp. 364, 394 f.

<sup>8</sup> Lammens, *op. cit.*, pp. 122 f.; cf. also *ibid.*, p. 109, n. 6, for other incidents.

<sup>9</sup> Ahmad ibn Ḥanbal, *Masnad* (Cairo, 1313), I, 332; Ibn 'Asākir, *Tārikh al-Kabir*, I, 367.

<sup>10</sup> Ibn Hishām, *Sirah*, ed. Wüstenfeld (Göttingen, 1859), pp. 132 f.

<sup>11</sup> *Ibid.*, p. 797.

<sup>12</sup> Ibn Sa'd, *Tabaqāt*, I, 49 f., 110, and 126.

<sup>13</sup> Cf. *Aghāni*, XXI, 275.

<sup>14</sup> Cf. Lammens, *op. cit.*, pp. 109, 135, 158.

<sup>15</sup> *Aghāni*, XXI, 206, lists a number of these; cf. also Anastase Marie de St. Elie, *op. cit.*, p. 60 and Freytag, *Arabum proverbia* (Bonn, 1838), I, 56, n. 1.

therefore also *rabbah*, underwent with the coming of Islam. For, whereas before Islam these words were applied to human beings, after Islam *rabb* was limited to the deity alone,<sup>16</sup> and *rabbah* was left with no further strictly religious Islamic use. Nevertheless, there seems to have been some two or three of these temple priestesses in Mohammed's time,<sup>17</sup> and one of them, Sarrā bint Nabḥān, is said to have been converted to Islam.<sup>18</sup> Arab prophetesses appear in the Moslem traditions even more rarely than do the priestesses. In fact, we know only of one active prophetess, the well-known Sajāh, whose prophetic career, to be dealt with below, ran its course largely after Mohammed's death.

We have seen how frequently the queens of ancient Arabia led their armies in person or accompanied their husbands on their campaigns. And what a queen does other women frequently do. It is, therefore, not surprising to find that the Arab woman on the eve of Islam played several roles in Arab warfare.<sup>19</sup> At times she was the cause, if not the prize, of intertribal warfare; at others she used her wits in providing a sort of intelligence service for the benefit of family and tribe. Frequently as not she accompanied her men "to the front," both to inspire and to help. Many a woman urged brother, husband, and son to heroic action; and there was no title that an Arab woman coveted more than that of *munjibah*, or "mother of heroes."<sup>20</sup>

Still this all important business of inspiring the warriors to courage, even unto death, was not left on major occasions to the inclination or patriotism of the individual woman. It was instead organized around a well-recognized institution that may well be called the cult of the Lady of Victory.<sup>21</sup> A woman of outstanding social position would be placed within or associated with the portable *qubbah*, or sacred pavilion, of the tribal or local deity. Other women, varying in number, would accompany her. The sacred group, within sight and hearing

<sup>16</sup> Lammens, *op. cit.*, pp. 134, 138f., 152, 154.

<sup>17</sup> *Ibid.*, p. 152 and n. 4.

<sup>18</sup> Ibn Sa'd, VIII, 227; cf. Gertrude H. Stern, "The First Women Converts in Early Islam," *Islamic Culture*, XIII (1939), 298.

<sup>19</sup> Cf. Ilse Lichtenstädter, *Women in the Aiyām al-‘Arab* (London, 1935), pp. 13 ff.

<sup>20</sup> Caussin de Perceval, *Essai sur l'histoire des Arabes* (Paris, 1847-48), II, 417; Nicholson, *A Literary History of the Arabs* (Cambridge, 1930), p. 88; cf. also Aghāni, XVI, 20.

<sup>21</sup> Cf. R. Geyer, "Die arabischen Frauen in der Schlacht," in *Mitteilungen der anthropologischen Gesellschaft*, XXXIX (1909), 148.

of the warriors, if not indeed in the actual fight from its start, urged and incited the men with their stirring war songs sung to the accompaniment of their lutes. The leader of the group was the Lady of Victory herself who, with hair flowing and body partly exposed, embodied an appeal to valor, honor, and passion. Around her and her women the battle raged until the day was lost or won. The practice no doubt had some age-long, though now perhaps somewhat dimmed, religious significance.<sup>22</sup> Its complex psychological influence on the warriors is not to be underestimated. For according to their military code the capture of the chief lady meant the loss of the battle to her side and the consequent disdain of the women for the vanquished fighters. For the captured women themselves it might mean slavery and dishonor. On desperate occasions, as, for instance, in the Battle of Dhū Qār,<sup>23</sup> the Arabs either hamstrung the camels carrying the women or severed their saddles and litters so that the women fell to the earth. This device of thus incapacitating the women at a time when they were exposed to extreme danger was meant to banish from the minds of the men any thought of retreat or flight. The warriors had to fight or die.

In still another role the women played an important part both during and after a battle.<sup>24</sup> They formed a sort of Red Cross base behind the immediate line of action, while the bolder ones ventured into these lines with water to quench the thirst of the fighters and with simple supplies with which to dress their wounds. After the battle the women, going out on the field now strewn with the fallen, the wounded, and the dying, concentrated on their ministering services to their own at the same time that, with the added equipment of a club, they dispatched the wounded enemy soldiers to their final rest. In a few instances some of these women, thirsting and literally hungering for revenge, would give vent to their emotions by barbaric mutilation of a corpse.<sup>25</sup>

With this brief sketch of woman's position and participation in public life, let us turn our attention to the women of the leading tribes

<sup>22</sup> Lammens, *op. cit.*, section on "Le Culte des Bétyles," esp. pp. 120-25.

<sup>23</sup> *Naqbāt id*, II, 643; for treatment of and references to several other incidents see Geyer, pp. 150 f.; Lichtenstädter, pp. 42 f.

<sup>24</sup> Geyer, pp. 152 f.; Lichtenstädter, p. 43.

<sup>25</sup> Geyer, pp. 152 f.

and families of Ḥijāz. Here again the Moslem traditions give us but bare glimpses of most of these women. But here and there an outstanding woman demanded and received their attention, though not infrequently the picture they give us of her is highly colored by the ideals and polities of her day and is sometimes even redrawn to harmonize with the policies of a later political party.

The first of the women of the Ḥijāz to come before us is Ḥubbā, the daughter of Ḥulail, the last Khuzā'ite priest-king of Makkah. The traditionally accepted story is that her father married her to the aggressive young Quṣayy whose Quraishite descent he accepted. To this couple were born four sons and two daughters, Takhmur and Barrah. Of the girls little is known, except that Takhmur is mentioned as mourning her father in verse.<sup>26</sup> The sons were ‘Abd al-Dār, ‘Abd Manāf, ‘Abd al-‘Uzzā, and ‘Abd Quṣayy—names famous in Quraishite genealogy and in early Islamic history. As Ḥulail advanced in age he turned over the guardianship of the Ka‘bah to his daughter Ḥubbā and handed her the key which she sometimes intrusted to her husband. Before he died he is said to have acknowledged his son-in-law, Quṣayy, and his own grandsons as his successors and to have left them his spiritual and temporal powers by a will, which, however, was repudiated or denied by the Khuzā'ah who thus forced Quṣayy and the Quraish to fight for their rights.<sup>27</sup> Other details, in contradiction with part of the above, are also woven into the story. Thus, when Ḥulail gave Ḥubbā the guardianship of the Ka‘bah, she pointed out to her father her inability to open and close the gates. Whereupon he appointed Abū Ghubshān (who according to some was his own son, but according to others not so) to do that for her. Later, evidently after Ḥulail's death, Quṣayy made Abū Ghubshān drunk and purchased his office from him for a skin of wine and some camels.<sup>28</sup>

Whichever version of this story we accept, the part played by Ḥubbā herself seems to be secondary. She seems to be willing to hand over the key to either her husband or her assistant, and she then dis-

<sup>26</sup> Ibn Sa‘d, I<sup>1</sup>, 39, 42.

<sup>27</sup> Ibn Hishām, 68, 75 f.; Ibn Sa‘d, I<sup>1</sup>, 37; Wüstenfeld, *Die Chroniken der Stadt Mekka* (Leipzig, 1857–61), I, 59 and 62 f., III, 44; Tabarī, I, 1092 ff.; Ibn Duraid, *Kitāb al-Ishtiqāq*, ed. Wüstenfeld (Göttingen, 1854), p. 276; cf. also Muir, *Life* (London, 1861), I, cc ff.; Caetani, *Annali dell'Islam*, I (1905), 99 ff.; Lammens, *op. cit.*, p. 112.

<sup>28</sup> Ibn Sa‘d, I<sup>1</sup>, 37; Tabarī, I, 1094; Mas‘ūdī, III, 117 f.

appears from the records. If either of the versions is to be accepted, Ḥubbā is only a means of transferring the control of the Ka‘bah from the tribe of Khuzā'ah to that of Quraish. On the other hand, if we do reject the story as a whole, there is nothing so improbable about its several details so far as Ḥubbā is concerned. As the daughter of a priest-king any woman might well be associated with some temple function; an ambitious and aggressive politician might well aspire to her hand and, having won it, use it to establish his own power and line. History is replete with the political marriage motif, and among the pre-Islamic Arabs themselves several instances are reported where political power was transferred from one house or dynasty to another through such marriages. There is, for instance, the case of the semi-legendary Balqis of the later Ḥimyārites and the Tanūkhid Jadhīmah of Ḥirah who married his sister to a descendant of the Abgarids who then succeeded him in power at Ḥirah.<sup>29</sup>

Aside from this connection of the office of guardianship of the Ka‘bah with Ḥubbā, who ought to be looked upon as a successor to or transmitter of her father's powers (and these, therefore, not necessarily limited to the guardianship of the Ka‘bah), the office itself is not associated with any other woman. Some modern scholars have drawn attention to the fact that at the time of the conquest of Makkah the key of the Ka‘bah was in the possession of a woman. They leave the impression that the sources themselves represent her as holding the key in her own right.<sup>30</sup> Actually, however, the traditions make ‘Uthmān ibn Ṭalhah, a descendant of Quṣayy through his son ‘Abd al-Dār, the one who really held the office of the guardianship. According to some, he seems to have intrusted the key to his mother, Sulāfah, for safekeeping. It was from him that Mohammed demanded the key and it was to him, ‘Uthmān, that his mother delivered it as it was again he, ‘Uthmān, who handed it over to Mohammed.<sup>31</sup> Sulāfah's part, so far as the traditions go, was apparently no more and no less than a mother's natural reluctance to see the key, and with it a lucrative office, pass from her son and family. When Mohammed, in ac-

<sup>29</sup> Ibn Hishām, *Kitab al-Tijān* (Hayderabad, 1928–29), pp. 144 ff.; cf. Nabia Abbott, *The Rise of the North Arabic Script and Its Kur‘ānic Development* ("OIP," Vol. L [Chicago, 1939]), p. 4.

<sup>30</sup> E.g., Lammens, *op. cit.*, p. 112; Stern, *op. cit.*, p. 298.

<sup>31</sup> Ibn Hishām, *Sirah*, p. 821; Wüstenfeld, *op. cit.*, I, 67, 184–87; Ya‘qūbī, *History*, II, 61.

cordance with his policy of leniency and reconciliation, returned the key and the office to ʻUthmān ibn Ṭalḥah, his mother, Sulāfah, did not figure in the scene at all. When one recalls that *kāhinah*'s, priestesses, and even a prophetess were on the scenes in the first years of Islam, one suspects that both Ḥubbā and Sulāfah have probably suffered considerable effacement at the hands of the Moslem traditionist.

So far as can be directly gathered from these same traditionists, the women do not seem to have been given any part in the government that Quṣayy eventually evolved at Makkah. The deliberations that were to be conducted in the *Dār al-Nadwah*, or council hall, were, with few exceptions, to be participated in only by the Elders or men of forty years and over. More often than not when the women of this period are mentioned in the traditions it is in connection with genealogy, since for this and the early Islamic period the mother's descent was as carefully investigated and as much valued as that of the father's. Here and there a woman is vaguely associated with some political affair. ʻĀtikah bint Murrah, wife of ʻAbd Manāf ibn Quṣayy and mother of several of his children including Hāshim, Muṭṭalib, and ʻAbd al-Shams, is credited with having a part in the *hilf al-Aḥabīsh*<sup>32</sup> or a confederacy bringing together the Quraish and the Abyssinian and negro elements in Makkah. The circumstances and date of this confederacy are more or less obscure.<sup>33</sup> It is, therefore, not surprising that ʻĀtikah's part in it is also obscure. The text can mean either that she brought about the alliance or that she participated in the concluding ceremonies. The former alternative seems hardly probable, and the nature of the latter can perhaps be guessed at by the part attributed to a daughter of ʻAbd Al-Muṭṭalib in the later *hilf al-Muṭaiyabīn*, or the "Confederacy of the Perfumed."<sup>34</sup> This confederacy was organized by the Banū ʻAbd Manāf against the party of the Banū ʻAbd al-Dār, when the latter refused to give up their prerogatives in the Ka'bah. In the concluding ceremony the parties to the confederacy, having first dipped their hands into a common bowl of perfume, placed them next on the Ka'bah to sort of sanctify the common pledge. Two of ʻAbd al-Muṭṭalib's six daughters are associated with this occasion. According to some, it was Umm Ḥakīm, but, according to others, it was her sister, ʻĀtikah (of whom more presently), who

<sup>32</sup> Yaʻqūbī, I, 279; cf. *EI*, I, 307 f.; art. "Hilf."

<sup>33</sup> Cf. Lammens, *op. cit.*, pp. 264–66.

<sup>34</sup> Cf. *EI*, I, 307 f., Arabic sources there cited.

prepared the bowl of perfume from which the confederacy took its name.<sup>35</sup> To whichever of the two sisters we concede the honor, the part itself so far as the traditions go seems to have been incidental.

We know next to nothing of Fāṭimah bint ʻAmr, one of several wives of ʻAbd al-Muṭṭalib, but mother of five of his daughters and three of his boys, including ʻAbd Allah and Abū Ṭālib, and therefore grandmother of both Mohammed and ʻAlī.<sup>36</sup> Four of her daughters, aunts though they were of Mohammed, are little more than names to us.<sup>37</sup> The fifth was ʻĀtikah, of whom the following story is told.<sup>38</sup> Three days before the arrival of Abū Sufyān's messenger at Makkah bringing the unexpected news of the threatened danger to his caravan she dreamed of this messenger and of the unwelcome message he had to bring. She was much alarmed and confided her dream to her brother ʻAbbās, who in turn confided it to a few others. Presently the dream was known all over the city and became a lively topic of conversation. Abū Jahl, Mohammed's inveterate enemy, seized the opportunity to taunt ʻAbbās with, "Is it not enough that your family has produced a prophet? Now you have a prophetess too! If nothing happens within three days, we shall be obliged to testify that you, the Banū Hāshim, are the greatest liars among the Arabs!" But the events, so the story goes, came to pass just as ʻĀtikah had dreamed them. She was converted, presumably soon after, and migrated to Madīnah. We hear little more of her, although she outlived Mohammed.

Yet another daughter of ʻAbd al-Muṭṭalib and aunt of Mohammed has received some considerable notice at the hands of the traditionists. She was Ṣafiyah, full-sister of Uncle Ḥamzah and mother of Zubair ibn al-ʻAwwām and therefore the grandmother of the future rival caliph ʻAbd Allah ibn al-Zubair. She seems to have been among the small group of early converts at Makkah and to have migrated early with the rest to Madīnah. At the Battle of Uhud (3/625) when Mohammed's forces were forced to retreat, Ṣafiyah, spear in hand, rushed among the soldiers, striking them and crying out scornfully, "So you desert the messenger of Allah!"<sup>39</sup> When the battle was over, Fāṭimah, we are told, dressed her father's wound, and Ṣafiyah searched for her

<sup>35</sup> Yaʻqūbī, I, 288, II, 16.

<sup>36</sup> Ibn Hishām, pp. 69 f.

<sup>37</sup> Cf. Ibn Saʻd, VIII, 27–31. Ibn Hishām (pp. 108–11) tells how all six daughters composed elegies on the death of their father.

<sup>38</sup> Ibn Saʻd, VIII, 29 f.; Ibn Hishām, pp. 428–30.

<sup>39</sup> Ibn Saʻd, VIII, 28.

brother Ḥamzah, to whom she was much attached. Mohammed, anxious to spare her the sad sight of Ḥamzah's mangled remains, told her son Zubair to head her off. But she would not be denied. "I will not go back," she cried, "until I see him." Leading her to the body, Mohammed left her to her anguished grief. Then to comfort her he told her that Ḥamzah's name was listed in paradise as the "Lion of Allah and of his Apostle."<sup>40</sup>

We find her again in an aggressive part during the siege of Madinah at the time of the Battle of the Trench (5/627). The women and children had been placed for safety in a fortlet belonging to Ḥassān ibn Thābit, Mohammed's court poet. Ḥassān, who fought for Islam with the pen but not the sword, was in the fort with this group. The Banū Quraizah, Jewish allies of Mohammed, were being courted by Abū Sufyān and were therefore becoming suspect to the Madinese, who were presently to accuse them of treachery, for which the unhappy tribe was later mercilessly massacred. While Mohammed still had his hands full with the enemy, Ṣafiyah, back in the fort, noticed a Jew prowling about. Suspecting him of being a spy, she asked Hassān to attack and kill him. But Ḥassān had no taste for the undertaking. So Ṣafiyah, taking a club, according to some, but a sword according to others, lay in wait for the Jew and, slipping through the gate, stealthily struck and killed him.<sup>41</sup>

In another and later scene she appears not quite in character with the fighting Ṣafiyah of the preceding incidents. The occasion was that of the conquest of Khaibar. In the usual challenge to single combat before the battle Zubair rushed forth to meet a Jewish challenger, whereupon Ṣafiyah, much alarmed, ran up to Mohammed and expressed her fears for her son's life. Mohammed assured her he would be victorious, Allah willing. Allah indeed so willed.<sup>42</sup> Allah's messenger then gave Aunt Ṣafiyah forty camel-loads of the produce of the newly acquired Khaibar while a similar gift went to his Aunt Ummāmah, who was also his mother-in-law, being the mother of his cousin-daughter-in-law and wife, Zainab bint Jahsh.<sup>43</sup> The last glimpse the traditions give us of Ṣafiyah is her presence, together with Fātimah, with the sick

<sup>40</sup> *Ibid.*, III<sup>2</sup>, 7-9 and VIII, 28; *Aghāni*, XIV, 23; cf. Muir, *Life*, ed. Weir (Edinburgh, 1923), p. 264.

<sup>41</sup> Ibn Hishām, p. 680; Ibn Sa'd, VIII, 27 f.; *Aghāni*, IV, 16; *Isābah*, IV, 671.

<sup>42</sup> Ibn Hishām, p. 761.

<sup>43</sup> Ibn Sa'd, VIII, 27 and 31. Cf. Ibn Hishām, pp. 773-75, for the long list of both men and women who received gifts on this occasion.

and dying Mohammed just before he was transferred to the house of Aishah. "Oh Fātimah, my daughter and thou Ṣafiyah, my aunt! Work ye out that which shall gain acceptance for you with the Lord; for I verily have no power with Him to save you in anywise."<sup>44</sup> She, with her sisters Arwā and Ātikah, composed several elegies on Mohammed<sup>45</sup> as did other women and men, the latter including Abū Bakr and the poet Ḥassān. Ṣafiyah herself died in the caliphate of 'Umar ibn al-Khaṭṭāb.<sup>46</sup>

Two women of the family of Abū Sufyān, chief of the Quraish at the time of Mohammed, have received considerable attention at the hands of the Moslem traditionists and historians. They were his wife, Hind bint 'Utbah, who opposed Mohammed and the new faith, and his daughter (not by Hind), Ramlah, who early accepted Islam and later married its prophet.

We do not know when Hind was born or when and to whom she was first married.<sup>47</sup> She was descended from the 'Abd al-Shams' branch of the Quraish, and we first meet her as the wife of Fākih ibn al-Mughīrah the Makhzūmite, the uncle of Khālid ibn al-Walid<sup>48</sup> the future "Sword of Islam." Fākih, suspecting Hind's fidelity and claiming to have seen a stranger leaving their private apartment, repudiated his wife and sent her back to her parents, thus exposing her to the active gossip of the community. Her father, 'Utbah, distressed at this development, was willing to go to any length to save his daughter's reputation. He, therefore, informed her that he had two alternative plans of action. If she was guilty, he would have Fākih assassinated and thereby silence his accusations; but, if she was innocent, then he would demand that Fākih submit the case to trial before some Yamānite *kāhin*'s. Hind vigorously protested her innocence, and the second plan was then followed with due publicity and ceremony. The *kāhin* pronounced Hind not guilty; furthermore, he prophesied that she would be the mother of a king named Mu'āwiyah. Fākih, accept-

<sup>44</sup> Ibn Sa'd, II<sup>2</sup>, 17, 46; cf. Muir, *op. cit.*, p. 494.

<sup>45</sup> Ibn Sa'd, II<sup>2</sup>, 93-97.

<sup>46</sup> *Ibid.*, VIII, 28.

<sup>47</sup> Ibn Sa'd, VIII, 170, does not mention her husband Fākih but says she was married to Hafṣ ibn al-Mughīrah, to whom she bore Abān; her marriage to Hafṣ is mentioned also by Ibn Qutaibah, *Uyun*, I, 283. Chronologically, this marriage seems to belong to the earlier career of Hind before her marriage to Fākih, unless there has been some confusion between Fākih and Hafṣ.

<sup>48</sup> *Aghāni*, VII, 26.

ing the decision, was more than willing to take her back, but she now would have none of him.<sup>49</sup>

How much of this story is fact and how much fiction can be left to the judgment of the reader. However, in its essentials this, the earliest story we have of Hind, characterizes her as a woman holding to the heathen practices of Arabia, a wife whose virtue was not above suspicion, and a proud lady with a mind of her own, a mind that was quick to decisive action. All three characteristics will cling to her throughout her career as we have it in the traditions.

Hind, thus parted from Fākih, had no lack of suitors. There was Musāfir ibn Abī ‘Amr, a minor Quraishite poet and a relative, who had developed a passionate love for her, and who now wished to marry her. Though he was well known for his poetry and generosity and held the respected title of "the traveler's provider," indicative of his great hospitality, Hind was satisfied with neither his position nor his wealth. She refused to marry him, though gossip again had it that she received him as a lover and that, fearing another scandal, she asked him to leave. Musāfir, still much enamored and hopeful, wended his way to Ḥirah to the court of the famed ‘Amr ibn Hind with the express purpose of seeking there fame and fortune with which to win his beloved to wife. Fate granted him the first of his wishes but ironically denied him the last for which alone the first was sought. For at Ḥirah Musāfir won ‘Amr's favor and prospered. But Hind either knew not of this or cared not to await the return of a distant lover. Two other suitors were at hand, and she was free to choose either, having first heard her father's description and estimate of them.<sup>50</sup> The one, Suhail ibn ‘Amr, she refused because, though he was noble, generous, and good tempered, he was withal a weak man. The second and successful suitor described in glowing terms as a man strong in character and in leadership, was none other than Abū Sufyān Ṣakhr ibn Ḥarb ibn Umayyah, the Quraishite, a wealthy and influential merchant if not the chief of his tribe and the actual leader of the city republic. The unfortunate Musāfir did not long survive the shock of this news casually imparted to him by none other than Abū Sufyān himself while on a trading trip to Ḥirah.<sup>51</sup>

It was soon after that Hind gave birth to Mu‘āwiyah, the future

<sup>49</sup> Aghāni, VIII, 50 f.; Ibn ‘Abd Rabbih, ‘Iqd al-Farid (Cairo, 1293), III, 273 f.

<sup>50</sup> Ibn Sa‘d, VIII, 171; ‘Iqd, III, 274 f.

<sup>51</sup> Aghāni, VIII, 49 and XIX, 105; Caussin de Perceval, I, 336–38.

caliph and founder of the Umayyad dynasty. The year of this event is not known; it is estimated as falling within the first decade of the seventh century of our era.<sup>52</sup> She bore Abū Sufyān one other son, ‘Utbah, and two daughters, Juwairiyah and Umm al-Ḥakam.<sup>53</sup> She was, of course, one of several wives of Abū Sufyān, though we hear little of her relationship with the other wives. Ibn Qutaibah relates how she constantly nagged Abū Sufyān until he divorced one of these, Sa‘bah, the daughter of ‘Abd Allah ibn Mālik.<sup>54</sup>

The traditions, so far, afford us only one definite reference to Hind for the entire period covering Mohammed's ministry at Makkah. Abū Lahab, the disapproving and actively antagonistic uncle of Mohammed, had already drawn a Qur'ānic curse on his head; so also had his wife, referred to as the "carrier of wood" in *Sūrah* 111, which is devoted entirely to the curse on this couple. Abū Lahab's family relations to Mohammed were further complicated by the fact that his son had married one of Mohammed's daughters, either Ruqaiyah or Umm Kulthūm. Abū Lahab's own wife, Umm Jamil, the sister of Abū Sufyān, disliked the girl and her father Mohammed and incited her husband against them. Abū Lahab forced his son to divorce Mohammed's daughter and when the sharp break came between Mohammed and his Makkan persecutors, who placed a ban and a boycott on the new movement, Abū Lahab deserted the Hāshimites and joined the opposition. Among the chief leaders of the opposition were several of Hind's relatives—including her father ‘Utbah and her uncle Shaibah and, of course, Abū Sufyān. Hind's own part at this stage of the opposition is not stated. That she must have been keenly interested in the current events is hardly to be doubted. Abū Lahab, meeting her on the street during the period of the ban, asked and received her approval of his conduct in the cause of Allāt and al-‘Uzzā and in the cause of those who followed them.<sup>55</sup>

But not all of Hind's family were opposed to Mohammed. Her brother, Abū Hudhaifah, was among the first converts to Islam. At the Battle of Badr (2/623) he challenged his father, ‘Utbah, to single combat; and Hind was quick to satirize him for this unfilial conduct.<sup>56</sup>

<sup>52</sup> Lammens, in *EJ*, III, 617.

<sup>53</sup> Ibn Qutaibah, *Kitāb al-Ma‘ārif*, ed. Wüstenfeld (Göttingen, 1850), p. 175; Ibn Sa‘d, VIII, 174 f.; *Iṣābah*, IV, 854.

<sup>54</sup> ‘Uyūn, IV (Cairo, 1930), 101; for Sa‘bah see *Iṣābah*, IV, 664.

<sup>55</sup> Ibn Hishām, 231. For Abū Lahab's family situation see *ibid.*, p. 465.

<sup>56</sup> Ibn Sa‘d, III<sup>1</sup>, 59 f.; cf. Muir, *Life*, pp. 60 and 228.

After the battle she satirized her cousin, Ramlah bint Shaibah, who had become Moslem and was married to 'Uthmān ibn 'Affān.<sup>57</sup> There was also later her stepdaughter, Ramlah bint Abī Sufyān, who married Mohammed.

It is in connection with the aftermath of the Battle of Badr that we get our first full view of Hind. But first we must take note of a curious story that connects her with the flight of Mohammed's daughter, Zainab.<sup>58</sup> Hind is here cast in the unexpected role of Zainab's helper, the story being traced back to Zainab herself. Hind, so the story goes, hearing of Zainab's plans to join her father at Madinah, offered to help her with supplies for the journey, justifying her unexpected action to the suspicious Zainab by a statement to the effect that the affairs of the men did not concern the women. When Zainab, starting on the journey, met with rough treatment at the hands of those who would prevent her departure, it was Hind who rebuked the ruffians with, "Ah! in time of peace ye are very brave and fierce against the weak and unprotected, but in battle ye are like women with gentle speeches." Hind's ready tongue may have uttered the gibe against Zainab's assailants; but it is difficult to account for her offer of help to Zainab. The account states that Abū Sufyān and the Quraish, having but recently suffered defeat and comparatively heavy losses at Badr, were lying low and were therefore ready to connive at Zainab's departure. Hind may, therefore, have been acting in accordance with this policy, but even then she would hardly need go to the length of offering direct help to Zainab. Further, if we accept this story, we would have to credit Hind with an amount of outward diplomacy that is incompatible with her general character, for her conduct in this instance is in conflict with the rest of her activities in this same period.

Hind, having lost father, uncle, and brother at Badr, took the affairs of men very much indeed to her woman's heart. Tradition credits her with several dirges on her father's death, though those learned in poetry are skeptical. However, her grief for her dead and her determination to avenge them are not to be questioned. Makkah, humiliated, was burning for revenge; but pride stifled the expression of natural grief. "Weep not for your slain," cried Abū Sufyān, "bewail not their

<sup>57</sup> Ibn Sa'd, VIII, 173 f.; Balādhuri, *Ansāb*, V, 106.

<sup>58</sup> Ibn Hishām, 466-68; Tabarī, I, 1348 f.; Muir, *Life*, pp. 345 f.; but see Henri Lamsens, *Fātimah et filles de Mahomet* (Rome, 1912), pp. 5-7, where Zainab's very existence is questioned.

loss; neither let the bard mourn for them. Show that ye are men and heroes! If ye wail and lament, and mourn over them with elegies, it will ease your wrath and diminish your enmity toward Mohammed and his followers. Moreover, if that reach our enemies' ears, and they laugh at us, will not their scorn be the severest calamity of all? Perchance ye may yet obtain your revenge. As for me, I will touch no oil, neither approach any woman, until I go forth to war against Mohammed." A month passed before their pent-up grief broke out with double force, and for another month all Makkah wept for its dead—except Hind. "Why sheddest thou no tears?" they asked of her; "why weepest thou not for thy father 'Utbah, for thy brother, and thine uncle?" "Nay," she replied, "I will not weep until ye again wage war with Mohammed and his followers. If weeping would wash away grief from my heart, I would weep even as ye; but it is not thus with Hind."<sup>59</sup>

Dwelling on her triple bereavement, Hind now saw fit to challenge the claim of al-Khansā of being the most bereaved of all the Arabs. Al-Khansā, who had lost her father and two full-brothers before Islam, had continued to mourn them in stirring and touching verse. It was her custom to visit the annual fair at 'Ukāz, riding in a pavilion marked by a banner, and there to give expression in verse to her grief. Hind, we are told, set out for the fair in a similar pavilion, sought out the poetess, and challenged her claim, whereupon both women composed extempore elegies on their distinguished dead.<sup>60</sup> No decision in favor of either is recorded, but the future was to accord the claim to al-Khansā, who lost four noble sons in the Battle of Qādisiyah.<sup>61</sup>

In the meantime the Quraish were preparing to avenge Badr and remove the stigma cast upon them by that defeat. Abū Sufyān, now the most prominent and acknowledged leader in Makkah, was foremost in these preparations, of which Hind no doubt approved. Furthermore, by virtue of her position as his wife, if not indeed by virtue of her own aggressiveness, she seems to have been accorded the role of leader among the women. The day of expected revenge drew near, and Hind and her women were ready.

<sup>59</sup> Muir, *Life*, pp. 236 f.; Wāqidi, *Kitāb al-Maghāzi*, ed. Kremer (Calcutta, 1856), pp. 14-18; this will be the edition cited unless Wüstenfeld's German translation is specifically indicated.

<sup>60</sup> Aghānī, IV, 34 f.; Cheikho, *Commentaries* . . . , pp. 58 f.

<sup>61</sup> Iṣābah, IV, 551; Cheikho, *Commentaries* . . . , pp. 21-23.

The Quraish, led by Abū Sufyān, marched to the Battle of Uhud (3/625) accompanied by their women and their gods in true heathen fashion. Most of the traditions enumerate the leaders as accompanied by their wives or mothers—Abū Sufyān, according to some, taking with him not only Hind but also a second wife, Umayyah bint Sa‘d.<sup>62</sup> Led by Hind, some fourteen or fifteen of these women, the cream of the Makkhan social aristocracy, played the time-honored role of Arab women in major battles. They sang their stirring war song, they played their tambourines, they danced and rushed onto the battlefield heedless of danger. Their men, seeing them, had to fight or die. In short, Hind and her women were the "Lady of Victory" group. And Hind played her part with amazing energy and spirit.<sup>63</sup> Nor were the rest of the women lacking in spirit or courage. When at one time the Quraish standard was down, it was one of them, ‘Amrah bint al-Hārith, wife of Ghurāb ibn Sufyān, who held it up.<sup>64</sup> When the battle was over and Quraish had won the day, Hind's long-awaited hour of revenge had come. It was not enough to know that Ḥamzah, who had killed her father at Badr, was dead by the hand of the Abyssinian, Abū Dasamah, whom she had specifically and repeatedly urged both before and during the battle to dispatch him, but she must herself see the fallen enemy and vent her wrath on his corpse. It is at this point that the traditions paint for us a particularly diabolic Hind. She is credited with tearing out Ḥamzah's liver and biting it, with cutting off his nose and ears, with accumulating on the field enough noses and ears to make necklaces, bracelets, and anklets which she wore with fiendish glee.<sup>65</sup> Then, standing on a high rock, she exultantly flaunted in the face of the fallen enemy the general victory and her personal revenge in extempore satirical verse which drew answer from the women in Mohammed's party and later from Ḥassān ibn Thābit.<sup>66</sup>

It is hard to believe that Hind, the aristocratic matron of Makkah, went to the barbaric revenge with which she is credited. Muir<sup>67</sup> long ago suggested that this picture of her as a Fury

<sup>62</sup> Ibn Hishām, p. 557; Wāqīdī, p. 201; Tabarī, I, 1385 f.; *Aghānī*, XIV, 12 f.

<sup>63</sup> Ibn Hishām, p. 562; Wāqīdī, pp. 207, 221; Tabarī, I, 1400 f.; *Aghānī*, XIV, 17.

<sup>64</sup> Wāqīdī, p. 201.

<sup>65</sup> Ibn Hishām, pp. 580–82; Ibn Sa‘d, III<sup>1</sup>, 5 f.; Wāqīdī, p. 279; Tabarī, I, 1415 f.; Ya‘qūbī, II, 48.

<sup>66</sup> Ibn Hishām, pp. 580–82; Tabarī, I, 1415–17; *Aghānī*, XIV, 20 f.; Ḥassān ibn Thābit, *Diwān*, ed. Hirschfeld (London, 1910), Nos. 214, 224 f.

<sup>67</sup> *Life*, III (London, 1861), 129.

was overdrawn by the traditionists, and Lammens<sup>68</sup> goes further and suggests that the account of her orgy was likely an ‘Abbāsid invention. Both of these suggestions I find are reinforced by a direct though hitherto overlooked statement of Ibn Sa‘d, who, after relating the incident, adds as his own opinion, "These are violent attacks on poor Hind."<sup>69</sup> It is interesting to notice in this connection that her son Mu‘āwiyah, founder of the Umayyad dynasty and hated alike by ‘Alids and ‘Abbāsids, is sometimes referred to in the traditions as the "son of the (human) liver-eater."<sup>70</sup>

Another story of Hind at Uhud seems likewise to bear the earmarks of an invention. It is that Abū Dujānah,<sup>71</sup> armed by a sword which Mohammed had given him, penetrated the enemy ranks and headed for the women. He reached Hind and held his sword over her head ready to strike but hesitated, unwilling to have it said that he had stained that sword with a woman's blood.

We hear little of Hind's doings until the next and final trial of strength between Mohammed and the Quraish. This was in connection with the conquest of Makkah in the Year Eight of the Hijrah. Here she appears in opposition to Abū Sufyān's policy of appeasement and surrender. Had she had the final word, she would have ordered the Quraish to give battle. When she realized that Abū Sufyān had practically handed the city over to Mohammed, her rage knew no bounds. She publicly denounced Abū Sufyān. Taking him by his beard and striking him with her hands, she cried out, "Kill this old fool, for he has changed his religion." Abū Sufyān himself the while was crying out, "O people, become Moslems and be saved!"<sup>72</sup> Again when ‘Abbās was proclaiming Mohammed's terms of safety, Hind strove to stir up the crowd against him.<sup>73</sup> But her efforts were of no avail. Realizing that the day, and with it the cause, was lost, she vented her wrath this time on her powerless gods. Shattering her idols to pieces, she cried, "We have certainly been deceived in you!"<sup>74</sup>

Tradition next has it that Mohammed had condemned five or six men and four women to death. Three of these were of the lower class, singing girls who had spoken evil of him. Two are believed to have

<sup>68</sup> *L'Arabie occidentale*, p. 124.

<sup>69</sup> Ibn Sa‘d, III<sup>1</sup>, 6.

<sup>70</sup> *Iqd*, II, 138; Ibn al-Tiqṭiqā, *Fakhrī*, ed. Derenbourg (Paris, 1895), p. 144.

<sup>71</sup> Cf. Caussin de Perceval, III, 101.

<sup>72</sup> Wāqīdī, pp. 308, 413.

<sup>73</sup> Ibn Hishām, p. 815.

<sup>74</sup> Wüstenfeld, *op. cit.*, I, 78; *Iṣābah*, IV, 821.

escaped the sentence; the third, according to some, is said to have paid the penalty.<sup>75</sup> The fourth on whom this sentence was passed was none other than Hind, and she escaped it by becoming Moslem and hastening, all muffled up, to take the oath of allegiance to Mohammed, who did not realize at first that he was indeed speaking to his erstwhile enemy, Hind.

If any incident in Hind's career is to be discredited, this certainly should be. It is inconceivable that Abū Sufyān, despite his differences with his wife, would consent to any agreement with Mohammed with a sentence of death hanging over Hind. Nor can we imagine Mohammed, with his farsighted policy of peaceful conquest and reconciliation, seriously considering such a sentence. Finally, Hind, in the taking of the oath of allegiance, speaks not as one fearing or sidestepping a death sentence but delivers herself with pride and spirit if not with veiled resentment.

Mohammed, having first received the oath of allegiance from the men of the now conquered city, turned his attention next to the women whose oath he deemed necessary for the full completion of the conquest and for the firm establishment of the new faith. Again, as at Uhud, Hind was the leader and the spokeswoman for the group. The scene, much dramatized by the historians, displays the Makkān women and their men, together with Abū Sufyān, at one side of the stage and Mohammed's party, including ʻUmar ibn al-Khaṭṭāb and ʻAbbās ibn ʻAbd al-Muṭṭalib, at the other, with Hind and Mohammed in the center. Mohammed began the administration of the oath:

*Thou shalt have but one God.*

"We grant you that."

*Thou shalt not steal.*

"I only stole provisions from Abū Sufyān, who is too stingy to give me enough." "That," said Mohammed, "is not theft."

*Thou shalt not commit adultery.*

"Does a lady commit adultery?"

*Thou shalt not kill thy children.*

"We brought up our young children but you killed them full grown at Badr."

<sup>75</sup> Ibn Saʻd II<sup>1</sup>, 98; Yaʻqūbī, II, 60 f.; Ibn Hishām, pp. 819 f.

*Thou shalt not slander.*

"Slander is indeed abominable and exceeds all bounds."

*Do not disobey me in anything that is right.*

"Had we intended to disobey you, we would not be here now."<sup>76</sup>

Hind was as good as her word. Thereafter she was on friendly terms with Mohammed, to whom on one occasion she is said to have brought a gift of two lambs<sup>77</sup> and to whom on other occasions she confided her economic difficulties with the close-fisted Abū Sufyān.<sup>78</sup> Later she was to fight as strenuously and wholeheartedly in the cause of Islam as she had previously fought against it.

It is not until early in the reign of ʻUmar ibn al-Khaṭṭāb that we hear again of Hind. She and Abū Sufyān were up in Syria visiting their son Muʻāwiyah, who was governor of that province. Both husband and wife took part against the Byzantines in the hard-fought Battle of Yarmūk (15/636), in which the Moslem women as a group fought with great energy. Hind's daughter, Juwairiyah, was among them and was wounded. Hind herself is cast in a characteristic role of leadership, her battle cry on this occasion being, "Strike the uncircumcised with your swords!"<sup>79</sup>

Some time after this Abū Sufyān divorced Hind,<sup>80</sup> but the reason is nowhere told. Both were by now pretty well advanced in age, Abū Sufyān being past seventy, since he died some sixteen to twenty years later aged eighty-eight years.<sup>81</sup> Hind, though past middle age, must have still retained her charm if the story that Muʻāwiyah refused her hand to an unnamed suitor is to be believed.<sup>82</sup> The divorce does not seem to have affected her sons' affection for her, for we find Muʻāwiyah and his brother ʻUtbah taking pride in being the sons of Hind.<sup>83</sup>

Hind took to trading after her divorce. She had no capital of her own and one would hardly expect Abū Sufyān, of whose stinginess she had complained repeatedly, to advance her any. We find her bor-

<sup>75</sup> Wāqidī, pp. 416 f.; Ibn Saʻd, VIII, 4; Tabarī, I, 1643 f.; Ibn al-Athīr, *Kāmil fi al-Tarikh* (*Chronicon*), ed. Tornberg (Upsaliae, 1851–76), II, 192 f.; *Fakhrī*, pp. 144 f.; *Iṣābah*, IV, 821. Cf. also Wensinck, *A Handbook of Early Muhammadan Traditions* (Leiden, 1927), p. 8, under heading, "Abū Sufyān: His Covetousness." These treaty clauses are found also in *Sūrah* 60:12.

<sup>77</sup> Ibn Saʻd, VIII, 171 f.; Ibn al-Athīr, II, 191.

<sup>78</sup> Ibn Saʻd, VIII, 172; reported frequently in the *Ṣaḥīḥain*.

<sup>79</sup> Balādhuri, *Futūḥ*, p. 135; Muir, *Annals* (London, 1883), p. 109.

<sup>80</sup> Tabarī, I, 2767.

<sup>81</sup> Nawawī, *Bioq. Dict.*, p. 726; cf. also *EI*, I, 108. <sup>82</sup> Tabarī, II, 69, 210; *Iqd*, II, 129.

rowing capital from the caliph ʻUmar and trading in the north in the lands of the Banū Kalb. Hearing that Abū Sufyān and his son ʻAmr were on a visit to Muāwiyah, she too made her way there to see Muāwiyah and to caution him against making lavish gifts to his father and half-brother lest the people be dissatisfied and complain of such gifts to the stern ʻUmar, who, being what he was, would not forgive him. Muāwiyah followed her advice and gave his father and brother a very modest gift, the size of which Abū Sufyān immediately attributed to Hind. The three traveled together to Madinah, where Hind disposed of her merchandise and where she had some argument with ʻUmar about the trade duties which he refused to remit since, he said, they belonged not to him personally but to the public treasury.<sup>84</sup> She seems to have sensed how futile it was to oppose the stern ʻUmar, for we find her on yet another occasion advising Muāwiyah to act in accordance with his wishes.<sup>85</sup>

Hind did not live to see Muāwiyah as caliph. According to some accounts, she died in the reign of ʻUmar in the year 14/635.<sup>86</sup> This must surely be an error, since she took part in the Battle of Yarmūk, at which time she was still the wife of Abū Sufyān. The story of her trading is placed by Ṭabarī under the year 23; this would fit in with the accounts that place her death in the reign of ʻUthmān.<sup>87</sup>

We have gone into the details of Hind's career for several reasons. Her own personal story, in what we can gather of it, is arresting. She might in a way be considered the last "queen" of pre-Islamic western Arabia. The traditional accounts of her story bear repeatedly the mark of later political coloring. In her private role as daughter, wife, and mother she claimed and exercised her rights as a free and spirited Arab woman. In her public role as the leading woman in the Makkān republic she was both a fearless though unheeded counselor and a ready fighter. Resenting the major role she, a woman, played against Mohammed, the Moslem traditionist of a later century, encouraged further by dynastic rivalries, caricatured the temperamental and aggressive Hind in the cause of Islam and to the detriment of the fallen Umayyads.

But Hind was not the only Arab woman to take a leading and ag-

<sup>84</sup> Tabarī, I, 2766 f.

<sup>85</sup> *Iqd*, II, 300.

<sup>86</sup> Masʻūdi, *Tanbih* ("BGA"), VIII (1894), 287; Ibn al-Athir, II, 380.

<sup>87</sup> *Iqābah*, IV, 821 f.

ressive part against Mohammed and Islam. The Banū Ghāṭafān resented Mohammed's encroachment on their territory to the northeast of Madinah. Their chief and leader was ʻUyainah ibn Ḥiṣn of the Fazārah who on several occasions clashed with Mohammed's men and with whom Mohammed finally made a treaty in order to keep the peace.<sup>88</sup> Again, during the siege of Madinah in the year 5/627, ʻUyainah's threatening opposition to Mohammed caused the latter to favor a treaty between them even to conceding ʻUyainah one-third of the produce of the date trees of Madinah. Fortunately for Mohammed there were those in his party who would not hear of this and who wished to give ʻUyainah "nothing but the sword."<sup>89</sup>

The next year we find Zaid ibn Ḥāritah, Mohammed's adopted son, on an expedition to Wādī al-Qurā against the Fazārah seeking to avenge a previous skirmish in which he had received a wound. He found the Fazārah on this as on the previous occasion led by the widow of Mālik ibn Ḥudhaifah, Umm Qirfah Fāṭimah bint Rabīʻah, a well-known and powerful woman though much advanced in age. She in person led her party including her numerous sons and grandsons against the enemy. But the day went against her; she and her beautiful daughter, Umm Ziml Salmā bint Mālik, were taken captive. Zaid avenged himself by putting the aged woman to a barbarous execution, tying each foot to a beast which when driven tore her in two.<sup>90</sup>

The defeat and drastic punishment may have had its influence on ʻUyainah since the dead Umm Qirfah was his aunt and the captive Salmā his cousin. At any rate we find him the next year in Mohammed's camp, though he was a follower with mental reservations. However, his influence was still so considerable that Mohammed counted him among those whose hearts must be won by generous gifts. He was, therefore, among those who received the largest share of the booty of Hunain, and one of those who refused to give any of it back to the unfortunate losers when these became Moslems. Distrusting this turbulent man, yet knowing his influence, Mohammed found it necessary to handle him with kid gloves.<sup>91</sup>

<sup>88</sup> Ibn Hishām, pp. 670, 719; Tabarī, I, 1463; Caetani, II<sup>1</sup>, 55 f. and 119; cf. also for brief biographies, *Iqābah*, III, 107–10; Nawawi, pp. 499 f.

<sup>89</sup> Ibn Hishām, p. 676; Muir, *Life*, pp. 307 and 312.

<sup>90</sup> Ibn Hishām, pp. 979 f.; Tabarī, I, 1557 f.; Wāqidī (Wüst.), pp. 236, 238 f.; Yaʻqūbī, II, 74 f.; Caetani, I, 700, 702.

<sup>91</sup> Ibn Hishām, pp. 874, 877 f., 881, 983, 988; Wāqidī, pp. 422 f.

But ʻUyainah had never been a true Moslem at heart. We find him, therefore, among those who apostatized after Mohammed's death. On his side, he claimed to have known little peace and to have experienced constant uncertainty regarding his boundaries ever since Mohammed came on the scene. The Ghāṭafān had in the past been allied with the Banū Asad, and ʻUyainah now renewed this alliance. This meant that he made his own the cause of the pretender, Ṭulaiḥah ibn Khuwailid of the Asad. This he was not unwilling to do, preferring to have a prophet from among his allies than one from the Quraish; for Mohammed was, after all, dead and Ṭulaiḥah very much alive. The news of this alliance caused several tribes to fall away from Islam and several others to watch and keep close to the new allies. Abū Bakr dispatched his ablest general, Khālid ibn al-Walid, against them. At the ensuing Battle of Buzākhah, ʻUyainah and his seven hundred fighters soon discovered that God was not on the side of Ṭulaiḥah. They retreated. Ṭulaiḥah and his wife escaped to Syria and later both accepted Islam. ʻUyainah was taken captive and brought to Abū Bakr. Accused of apostasy he boldly exclaimed he was never a Moslem until then and thus won a pardon.<sup>92</sup>

But not all of Ṭulaiḥah's party were willing to give up the fight. With their previous leaders deserting them, the small group of die-hards found a desperate and daring leader in none other than Salmā, cousin of ʻUyainah and daughter of the barbarously executed Umm Qirfah. Salmā, whom Mohammed managed to acquire from her captor in the affair of Wādi-al-Qurā, had been given to Aishah, whom she served for a time. Later she was married to a relative of Mohammed. She had gone over with ʻUyainah to Ṭulaiḥah and, remembering her mother's cruel fate, determined now to avenge it or die. To her banner flocked men of Fazārah, Asad, Hawāzin, Sulaim, and Tayy. Like her mother, she led her men in person, riding on her mother's camel. Around her the fighting was most severe, for Khālid had promised a hundred camels to him who should disable hers. The odds were heavily against her; she and her camel fell but not before a hundred others (*sic!*) had fallen around her.<sup>93</sup>

<sup>92</sup> Balādhuri, *Futūb*, p. 96; Tabarī, I, 1893, 1896; Yāqūt, *Geog.*, I, 601

<sup>93</sup> Tabarī, I, 1901–3; Yāqūt, *Geog.*, II, 353; *Sirat al-Hallabiyah*, III, 203–5; *Iṣābah*, IV, 638 f., calls her Umm Qirfah the younger and makes her erroneously the granddaughter of Umm Qirfah the elder. Caetani (II<sup>1</sup>, 623 f. note, and II<sup>2</sup>, 811) questions this episode

It was this same period of the apostasy of the Year Eleven that brought the last of the Arab "queens" to the foreground. This was the prophetess Umm Ṣādir Sajāh bint Aws ibn Hiqq of the tribe of Tamīm. Her story is nowhere fully and consecutively told. We know nothing of her before her sudden appearance and little that is certain after that, while of the major episode itself in which she figures several versions are told.<sup>94</sup> Of these, two are more or less clearly defined. The one originating from the school of Ḥirāq and transmitted largely through Saif ibn ʻUmar, a fellow-Tamīmite living in the time of the Abbāsid Hārūn al-Rashid, and supplemented by sundry notices, gives us a very ungratifying picture of Sajāh as a woman and as a politico-religious leader. However, this same Saif, thanks to Western scholarship, has been shown to be a notorious romancer whose great objective was the glorification of his tribe and the removal from it, if possible, of the guilt of apostasy.<sup>95</sup> As for the sundry notices, they bear every mark of wilful malice. According to this version, therefore, Sajāh was a Tagħlibite and not a Tamīmite; she came from Ḥirāq and not from the Tamīm territory; she was a weakling whom the aged Musailamah, the false prophet, violated; she was a pretender impressed with Musailamah's second-rate and insipid if not vulgar utterances. In short, she was a weak woman and a false prophetess who eventually saw the true light and died a good Moslem.<sup>96</sup>

The other and less biased version comes from the school of Madīnah.<sup>97</sup> Supplementing it with other notices and bearing in mind the general situation, it is possible to piece together the most probable story of Sajāh as follows. She was herself a Tamīmite, though her mother was of the Banū Tagħlib, which tribe had long settled in southern Ḥirāq and was largely Christianized. In all probabilities Sajāh's religious ideas were influenced by those of her mother's

because it is received on the sole authority of Saif. However, it is difficult to see what motives Saif could have had for its fabrication. The elements in the story itself and Umm Ziml's backgrounds make the episode probable, though perhaps somewhat embellished by Saif.

<sup>94</sup> Caetani, II<sup>1</sup>, 626–35, 644–48; II<sup>2</sup>, 809–12, gives the best and most detailed account of Sajāh, which is largely followed in the brief notice of her in *EI*, IV, 44 f.

<sup>95</sup> Cf. *EI*, IV, 44, and Wellhausen, *Skizzen und Vorarbeiten*, VI (Berlin, 1899), 3 ff.

<sup>96</sup> Tabarī, I, 1908, 1911–19; Aghāni, XVIII, 166 f.; cf. Caetani, II<sup>1</sup>, 628 ff.; *Fakhrī*, pp. 104 f.

<sup>97</sup> Balādhuri, *Futūb*, pp. 99 f.; cf. Caetani, II, 626 f.

tribe, though there is nothing to make us conclude that she herself was a Christian. Some of the accounts tell us she was a *kāhinah*.<sup>98</sup> We have already seen the significant role a *kāhin* or *kāhinah* could and did play in heathen Arabia. *Kāhinah* or not, Sajāh must have already achieved some sort of leader's position with her people before the death of Mohammed, otherwise she could not have found such ready followers when, on Mohammed's death, she proclaimed herself a prophetess.

The politico-religious situation into which she plunged and on which she risked her political all was indeed a complicated and serious one. Mohammed was dead. Al-Aswad, Tulaiḥah, and Musailamah had come forth as prophets. This then was an age of prophets. A stout-hearted, ambitious woman, especially if she were already a *kāhinah*, would find the situation inviting, and into the arena of prophets would come a prophetess. Sajāh must have thought herself equal to the challenging situation.

Several of the divisions of the Banū Tamīm threw off the Moslem faith and cast in their lot with hers; among them were the Hanzalah subtribes with their leaders Wakī ibn Mālik and Mālik ibn Nuwairah. Other divisions hesitated between Islam and apostasy. The Banū Tamīm were thus divided. This led to civil war among them, and Sajāh's faction lost two minor battles, after which some of her subtribes deserted. She was thus forced to make peace on condition she leave the Tamīm territory. This setback was not serious enough to cause her to forsake the call to prophesy. She would march against the prophet Musailamah and, having vanquished him, try to settle matters with Abū Bakr and the Quraish. For were not the Tamīm, like the Quraish, of the Maḍr Arabs? If prophecy can belong to the latter, it can also be the right of the former. The Quraish and Mohammed can stay in the Ḥijāz and northwestern Arabia; the Tamīm and Sijāh claimed the Najd and northeastern Arabia.<sup>99</sup>

Sajāh and her army headed for Yamāmah, Musailamah's capital. Here, local rivalry and opposition to Musailamah had crystallized around Thumāmah ibn Uthāl, and Sajāh's advance came, therefore, at a most inopportune moment. Musailamah, taking council with his men, decided to meet her halfway and work out a peaceful arrange-

<sup>98</sup> Balādhurī, *op. cit.*, p. 99.

<sup>99</sup> Cf. *Aghāni*, XIV, 66; XVIII, 165 f.

ment. An interview was arranged, the result of which is variously told. It was at this point that the aged—he was said to have been a hundred and fifty years old—and distracted Musailamah is supposed to have assaulted the warring Sajāh, forced her into a dishonorable marriage, which he promptly repudiated, and then sent her humiliated, disgraced, and empty-handed back to her people in southern ‘Irāq.<sup>100</sup> Utter nonsense! Another version states that at this interview Musailamah succeeded by his timely revelations in convincing Sajāh of his superior claim to prophecy and then proposed honorable marriage which she accepted. With this version would fit the tradition that Sajāh made common cause with Musailamah, with whom she stayed until the fall of Yamāmah.<sup>101</sup> Some of the accounts, in addition to one or the other of the above versions of this interview, mention a treaty between Sajāh and Musailamah, according to which she was to withdraw her army in return for a year's revenue from Yamāmah. Half of this revenue was to be paid then, and the other half was to be turned over later to three of her generals whom she left behind for that purpose.<sup>102</sup>

Were it not for Musailamah's extreme age and the mention of a treaty, the second version of the marriage might find some acceptance. As it is, the probabilities are in favor of the treaty alone. For, in the meantime, the die was being cast in favor of Quraish. Khālid had already made swift work of Tulaiḥah and perhaps also of Umm Ziml Salmā. Sajāh's own army was none too good or reliable. Musailamah's terms were generous. Sajāh might well have decided that under the circumstances discretion was the better part of valor. She accepted the terms and went home, not to the Tamīm territory from which she had been recently ousted but to her maternal uncles, the Taghlībs of ‘Irāq. If she had any further claims of religious leadership and other ambitious plans of military conquest, the events of the succeeding months must have put an end to them. Khālid, the Sword of Islam, had swept like a scourge over the land. Mālik ibn Nuwairah, who had been her chief Tamīmite ally, was no more; and Musailamah, the last and most pretentious of the “false” prophets, had fallen in the

<sup>100</sup> *Aghāni*, XVIII, 166 f.; *Fakhrī*, pp. 104 f.

<sup>101</sup> *Ṭabarī*, I, 1917–19; *Ya‘qūbī*, II, 144; *Abū al-Fidā*, I, 208–12.

<sup>102</sup> *Ṭabarī*, I, 1919 f.; *Ibn al-Athīr*, II, 271.

disastrous Battle of 'Aqrabah. The balance of the revenue he had promised her was already in the hands of the conqueror who was, moreover, pushing further and further toward 'Irāq. Is it any wonder, then, that we hear no more of Sajāh, the warrior-prophetess? Did she stay with the Taghib and become Christian? Or did she return to the Tamīm, now united once again in their faith, and, like them, become a Moslem? In the latter case she would be but following in the footsteps of Tulaiḥah. Most of the traditions tell briefly that she, in time, returned to the Tamīm, became a Moslem, and settled and died in Basrah, which had become the chief Tamimite city under Mu'āwiyah, who settled that tribe there in 41/661-62.<sup>103</sup>

The Moslem traditions have preserved nothing for us of Sajāh's teaching. Only a few of her rhymed utterances have survived, and these are mostly orders to march to battle. Her deity is referred to as *Rabb-al-Sihāb*, "The Lord of the Clouds." She had her *mu'adhdhin*'s or men that called the people to prayer; she was attended by a *hājib* or chamberlain and is said to have delivered her message from a *minbār* or pulpit.<sup>104</sup>

Emerging from an almost complete obscurity, Sajāh, the warrior-prophetess, played a brief but major role on the political stage of Central Arabia and then stepped off that stage into an even more dense obscurity than that out of which she had first emerged. Yet, in her, the contemporary feminine counterpart of Mohammed the prophet-king, we see the last of that ancient and long though repeatedly broken line of independent Arab queens. The Arab lands that once produced the Queen of Shebah, the empress Julia Domna and the illustrious Queen Zenobia, will know no more independent and warring Arab queens. Henceforth a favorite "consort," an aggressive "queen-mother," or even an ambitious sister or aunt, will pull some political strings. A few of these will be so adroit at this performance that the political stage of their day will present us with nothing but a clever woman's puppet show. However, independent Moslem queens will emerge later, but they will not be the daughters of Arabia.

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<sup>103</sup> Tabarī, I, 1920; cf. Caetani, II<sup>1</sup>, 648; Ibn al-Athīr, II, 271.

<sup>104</sup> EI, IV, 44.

## PERSIAN AND EGYPTIAN CHRONOLOGY

RICHARD A. PARKER

Under this heading are here presented three studies in the chronology of Egypt during the first Persian domination. When Persian chronology itself is touched upon, it is, in the main, from the standpoint of the Egyptian evidence. Interest in these problems followed upon the invitation of Professor A. T. Olmstead to join in the work of his seminar in near eastern history for the year 1940-41.

### I. THE ACCESSION DATE OF DARIUS I

Recent articles in this *Journal* by Professors Olmstead and Poebel have once again directed attention to the question of the accession date of Darius I.<sup>1</sup> The problem arises from the fact that two interpretations of the length of the reigns of Bardiya and Nebuchadnezzar III have been drawn from the dates by which Babylonian tablets are dated. Thus, if Bardiya ruled not seven months but a year and seven months, Darius' accession must be placed not late in 522 b.c., the usually accepted date, but in 521; and if Nebuchadnezzar III ruled not three months but nearly a year, it must be further dropped to 520, his first year, then, being 519/18.<sup>2</sup>

Consideration of the Egyptian data which bear on the problem has led me to the conclusion that the traditional date of 522 for Darius' accession is correct and that, no matter how one may be inclined to interpret the tablet material, it must be accommodated to that date. Decisive evidence for my conclusion is found in the double-dated Aramaic papyri from Egypt; but, before dealing with them, it may be appropriate to discuss briefly the other Egyptian material which has

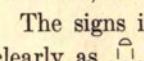
<sup>1</sup> A. T. Olmstead, "Darius and His Behistun Inscription," *AJSL*, LV (1938), 392 ff.; Arno Poebel, "The Duration of the Reign of Smerdis, the Magian, and the Reigns of Nebuchadnezzar III and Nebuchadnezzar IV," *AJSL*, LVI (1939), 121 ff.; cf. also his earlier articles, "The Names and the Order of the Old Persian and Elamite Months during the Achaemenian Period," *ibid.*, LV (1938), 130 ff.; "Chronology of Darius' First Year of Reign," *ibid.*, pp. 142 ff. and 285 ff.

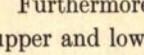
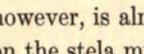
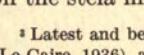
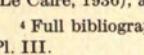
<sup>2</sup> It would not be possible to retain 522 for the date of Darius' accession and push back the reign of Cambyses by two years, for his seventh year is fixed astronomically by an eclipse of the moon to 523/22 b.c. (cf. n. 26 below).

been thought to be useful evidence. This last consists of the Apis stela of year 4 of Darius I, and one of the several Wadi Hammamat inscriptions of a Persian official, Atiyawahy.<sup>3</sup>

#### A. THE APIS STELA OF YEAR 4 OF DARIUS I<sup>4</sup>

This stela gives the dates of the birth, death, and burial of Apis and the length of his life. As he was born in the fifth year of Cambyses, Tybi 29, and died in the fourth year of Darius, Pachons 4, the length of his life ought to be significant. The number indicating this has been read as 7 years, 3 months, and 5 days by some scholars,<sup>5</sup> but as 8 years, 3 months, and 5 days by others.<sup>6</sup>

The signs in question, with the *rnp*-sign to the right, appear clearly as . Borchardt says: "Die 7 besteht aus vier Strichen oben und drei, die auf Lücke stehen, darunter. Der schwache Strich in etwas grösseren Abständen links von diesen drei Strichen ist nicht beabsichtigt."<sup>7</sup> This left lower stroke, however, does not appear to be at all "weak," though it is not so evenly spaced as the others. Posener states, without qualification: "La position de  $\Delta$  confirme la lecture 8."<sup>8</sup> Why he should be so certain is not clear. The  $\Delta$  does not extend above the stroke in question, nor is it over the center of the eight strokes. He may perhaps mean that it is nearer a central position over the eight than the seven strokes, and a central position would be the objective in good Egyptian writing.

Furthermore, while not invariable, the normal writing of 7 with upper and lower strokes is ; rarely it is  or . Eight, however, is almost invariably . The eight strokes as arranged on the stela might thus be considered as an error for 7 (this must, of

<sup>3</sup> Latest and best publication in G. Posener, *La première domination perse en Egypte* (Le Caire, 1936), a valuable sourcebook for this period.

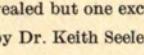
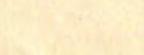
<sup>4</sup> Full bibliography, translation, commentary, and photograph in *ibid.*, pp. 36 ff. and Pl. III.

<sup>5</sup> Wiedemann, *Geschichte Ägyptens* (1880), p. 219; Borchardt, *Die Mittel zur zeitlichen Festlegung von Punkten der ägyptischen Geschichte und ihre Anwendung* (Kairo, 1935), p. 64. Wiedemann's reading is accepted by Poebel, "The Duration of the Reign of Smerdis . . . ." *op. cit.*, pp. 128-29.

<sup>6</sup> Revillout, *Notice des papyrus démotiques archaïques* (Paris, 1896), pp. 387-88; Chassinat, *Recueil de travaux*, XXIII (1901), 77-78; Gauthier, *Le Livre des rois d'Egypte* (Le Caire, 1915), IV, 138, n. 1; Posener, *op. cit.*, p. 39.

<sup>7</sup> *Op. cit.*, p. 64, n. 3.

<sup>8</sup> *Op. cit.*, p. 38 (m).

<sup>9</sup> In Medinet Habu III: *The Calendar*, etc., a text in which carelessly written figures abound, in addition to numerous examples written as above a somewhat hasty search revealed but one exception,  on Pl. 156, l. 867. Another was later pointed out to me by Dr. Keith Seele,  on Pl. 165, l. 1359.

course, be our eventual conclusion) or, possibly, as an unconventional rendering of 8, explained as a graver's correction of an original 7. As it stands, however, the stela must be considered quite indecisive as evidence.<sup>10</sup>

#### B. THE WADI HAMMAMAT INSCRIPTIONS OF ATIYAWAHY<sup>11</sup>

Atiyawahy (*Atiywh*) was a  of Persia and governor of Coptos who made numerous visits to the Wadi Hammamat quarries. Records of seven such visits have been found. One of them, No. 28, has been considered by Wiedemann<sup>12</sup> to give the lifetime of Atiyawahy as six years under Cambyses, thirty-six under Darius, and twelve under Xerxes—an assumption which, if correct, would demonstrate that Darius' first year followed the last of Cambyses without any interval. Posener, on the other hand, believes that this inscription refers to two different preceding visits and not to the lifetime of Atiyawahy at his last visit.<sup>13</sup> In this he is certainly correct, as a consideration of the other inscriptions demonstrates. Let us list their essentials.

- |     |  |
|-----|--|
| No. |  |
| 24. | Year 36 of . . . Darius . . .  |
| 25. | Year 2, 1st month of the 1st season, day 19 of . . . Xerxes . . .                |
| 26. | Year 6 of . . . Xerxes . . .   |
| 27. | Year 10 of . . . Xerxes . . .  |
| 28. | Year 6 of . . . Cambyses; year 36 of . . . Darius; year 12 of . . . Xerxes . . . |
| 29. | Year 12 of . . . Xerxes . . .  |
| 30. | Year 36 of . . . Darius . . . ; year 13 of . . . Xerxes . . .                    |

All the above are followed by the line: "Made by (*ir.n*) the *srs* of Persia, Atiyawahy." Wiedemann would understand all these inscriptions to record, not "Year x," but rather "X years," lived (made) by Atiyawahy.

<sup>10</sup> Posener's objection (*op. cit.*, p. 40) to the use of this stela for chronological purposes, from the standpoint that Egyptian dating differed from Persian, will be met in Part III of this article. It may be said here, however, that it does not apply.

<sup>11</sup> Full bibliography in Posener, *op. cit.*, pp. 117 ff., his numbers 24 to 30. References are there given to photographs in Couyat-Montet, *Inscriptions du Ouadi Hammamat* (1912). For convenience I shall refer to the individual inscriptions by Posener's numbers.

<sup>12</sup> *Op. cit.*, pp. 220-22; quoted by Poebel, "The Duration of the Reign of Smerdis . . . ." *op. cit.*, p. 129.

<sup>13</sup> *Op. cit.*, p. 123.

Reading the initial signs, which are  $\{\circ\}$ , as *rnpt*, "year," instead of *h3t-sp*, "regnal year," is a priori most improbable. We know from any number of other examples that inscriptions in the Wadi Hammamat record a trip made to the quarries at a certain time by a certain official or group of officials. Such, for example, must be No. 25 above. To interpret that as "1 year, 19 days under Xerxes lived by Atiyawahy" would be an impossible strain on the Egyptian. This inscription merely records a date, a point in time and not a period of time, and so too must all the others, even though only to the year. The fact that Darius reigned thirty-six years and Atiyawahy visited the quarries in *year 36* need be nothing more than a coincidence. Again, if No. 28 were intended to record the life-span of Atiyawahy, why should Nos. 24 and 30, which also mention *year 36* of Darius, fail to include *year 6* (or six years) of Cambyses?

It must be admitted that the recurrence of *year 36* in later inscriptions is puzzling. Atiyawahy must have attached to it some special significance. One explanation of its prominence might be that it was his first official trip and that, as such, he commemorated it later. It is to be noted that during the earlier years of Darius we have inscriptions at the quarries made by an Egyptian director of work, Khnemibre, whose last date is in *year 30*.<sup>14</sup> Atiyawahy would have taken over between then and *year 36*.

*Year 6* of Cambyses, in No. 28, may commemorate another date in Atiyawahy's life which he thought important—perhaps his birth, perhaps a first trip as a boy. Posener has pointed out that, if he were twenty years old at the time, in *year 12* of Xerxes he would have been around seventy.<sup>15</sup> This is a not impossible age, but it seems more likely that he was younger.

The Egyptian evidence we have thus far discussed has been seen to have slight value, but we may now turn to another body of material which will be found to have much greater significance.

#### C. THE ARAMAIC PAPYRI FROM EGYPT

Among the Aramaic papyri of the Persian period from Egypt are a number bearing double dates.<sup>16</sup> It is now generally accepted that these

<sup>14</sup> Posener, *op. cit.*, Nos. 11–23.

<sup>15</sup> *Ibid.*, p. 123.

<sup>16</sup> A. H. Sayce and A. Cowley, *Aramaic Papyri Discovered at Assuan* (London, 1906); A. Cowley, *Aramaic Papyri of the Fifth Century, B.C.* (Oxford, 1923).

dates are according to the Babylonian and the Egyptian calendars;<sup>17</sup> this being so, they become most valuable data for checking Persian chronology.<sup>18</sup> It has long been possible to convert Egyptian dates of this period into the Julian calendar with no error by the use of Eduard Mahler's *Chronologische Vergleichungs-Tabellen* (Wien, 1889). More recently, the same operation with Babylonian dates has become practicable, with a rare uncertainty of one day.<sup>19</sup> For our purpose this uncertainty is insignificant.

As a hypothetical example, let us suppose that we have a papyrus dated "Abu 1 = Pachons 9, year 11 of King X," and we know that "year 11 of King X" might be any year from 456 to 452 B.C. In order to determine the exact year, we calculate the Julian dates for Abu 1 and Pachons 9 for all five years:

Abu 1 = August 11	Pachons 9 = August 20, 456 B.C.
" " = " 1	" " = " " 455
" " = " 20	" " = " " 454
" " = " 8	" " = " " 19, 453
" " = July 28	" " = " " 452

It is evident at once that 454 B.C. must be the correct year, and it is obvious that one day one way or the other in any of the Abu 1 dates would not invalidate this conclusion.<sup>20</sup>

<sup>17</sup> M. H. Pognon, "Chronologie des papyrus araméens d'Eléphantine," *Journal asiatique*, XVIII (10th ser., 1911), 337 ff.; Eduard Mahler, "Die Doppeldaten der aramäischen Papyri von Assuan," *ZA*, XXVI (1912), 61 ff.

<sup>18</sup> Not until the writer had completed his work did he discover that he had been anticipated, both in his use of the Aramaic papyri and in his conclusions, by Professor Martin Sprengling in an early article, "Chronological Notes from Aramaic Papyri," *AJSL*, XXVII (1911), 233 ff. The fact that a second year following the traditional date must be considered, and our present much greater knowledge of the Babylonian calendar, justify him, he feels, in a new treatment of the subject.

<sup>19</sup> The writer, in collaboration with Dr. Waldo H. Dubberstein, is now engaged in compiling tables of Julian dates for the beginning of every Babylonian month from 588 B.C. to the Christian Era. They are to be incorporated in a forthcoming study of the Neo-Babylonian calendar. The Babylonian dates in this article are based on this unpublished material, but they may be roughly checked, if desired, by the early tables of Eduard Mahler, *Zur Chronologie der Babylonier* ("Denks. d. kais. Akad. d. Wiss., math. naturw. Cl.", Vol. LXII [Wien, 1895]), pp. 641 ff., controlled by the dates for Nisanu 1 in D. Sidersky, "Contribution à l'étude de la chronologie néo-babylonienne," *RA*, XXX (1933), 57 ff.

<sup>20</sup> Other points to be considered are: (a) Babylonian days ran from sunset to sunset and Egyptian days from daybreak to daybreak, so that, if a papyrus were written in the evening, the Babylonian date would be one day later than the Egyptian. (b) The new moon was visible at Assuan forty-seven minutes earlier than at Babylon. As the new month began with the first appearance of the crescent, upon occasion a month could begin at Elephantine one day before it began at Babylon. Apart from all these considerations, mere scribal errors in dealing with two calendars are undoubtedly present.

Seven papyri have their dates sufficiently well preserved to be usable for our present purpose.<sup>21</sup> They are tabulated according to the following scheme. The first regnal year worked out for each papyrus is the traditional date, calculated from the Ptolemaic canon, according to which the first years of the reigns of the six kings from Cambyses to Artaxerxes II are as follows:

	Years of Reign	Date of First Year (Not Accession Year)
Cambyses.....	8	529/28 B.C.
Darius I.....	36	521/20
Xerxes.....	21	485/84
Artaxerxes I.....	41	464/63
Darius II.....	17	423/22
Artaxerxes II.....	46	404/3

Following this traditional year are the Julian dates for the next two years, into which the given regnal year might fall according to the chronology resulting from a longer reign for Bardiya or Nebuchadnezzar or both.

1. Cowley 5 = Sayce and Cowley A

Ululu 18	= Pachons 28	Year 15 of Xerxes
September 13	= September 12/13	471 B.C.
October 2	= " "	470
September 21	= " 11	469

2. Cowley 6 = Sayce and Cowley B

Kislimu 18	= Thoth 17 or 14 <sup>a</sup> or 17 <sup>b</sup>	Year 21, the beginning of the reign when King Artaxerxes sat on his throne <sup>b</sup>
January 3	= December 23	January 2 465/64 B.C.
December 23	= " "	" 464/63
" 12	= " "	" 463/62

(a) Cowley states that there is hardly room for 17. This papyrus has been included in spite of the uncertainty of the Egyptian date because of the importance of the year dating, which shows that year 21 of Xerxes was also the accession year of Artaxerxes.

(b) As will be explained in Part III, the scribe normally used double-year dates after Thoth 1 until the Egyptian and Persian years again coincide.

<sup>21</sup> One other (Cowley 8 = Sayce and Cowley D) has its dates preserved, but they are demonstrably incorrect (cf. Mahler, ZA, XXVI [1912], 64).

cided, after Nisanu 1. Thus we should expect here, "Kislimu 18, year 21 = Thoth x, year 22, the beginning of the reign, etc." However, for a long period up to one or two weeks before, the scribe had been accustomed to write only "year 21," and he may easily have forgotten the present necessity for a change, just as we ourselves frequently fail to write the correct year in January dates. Moreover, the scribe was undoubtedly confused by the fact that the accession year of the next king was involved.

3. Cowley 10

Kislimu 7	= Thoth 4	Year 9 of Artaxerxes I <sup>a</sup>
December 14	= December 18	456 B.C.
" 3	= " "	455
" 22	= " "	454

(a) Here again we should expect "Kislimu 7, year 9 = Thoth 4, year 10"; but see note (b) to No. 2 above.

4. Cowley 13 = Sayce and Cowley E

Kislimu 3	= Mesore 10	Year 19 of Artaxerxes I
November 19 <sup>a</sup>	= November 17	446 B.C.
December 7	= " 16	445
November 27	= " "	444

(a) Kislimu 3 in Babylonia was probably December 19, as the nineteenth year of Artaxerxes I should have a second Ululu (cf. the forthcoming study of the Neo-Babylonian calendar by the writer and Dr. Waldo H. Dubberstein). By this time the nineteen-year cycle with six Addaru's and one Ululu as intercalated months is fairly well attested, and the failure of the scribes in Elephantine to know or to recall that this particular year required an Ululu and not an Addaru can easily be explained by their isolated position geographically.

5. Cowley 14 = Sayce and Cowley F

Abu 14	= Pachons 19	Year 25 of Artaxerxes I
August 27	= August 26	440 B.C.
" 16	= " "	439
" 6	= " "	438

6. Cowley 25 = Sayce and Cowley J

Kislimu 3	= Thoth 12	Year 8/9 of Darius II <sup>a</sup>
December 17	= December 16	416 B.C.
" 6	= " "	415
November 25	= " "	414

(a) That is to say: year 8 of the Babylonian calendar is year 9 of the Egyptian.

## 7. Cowley 28 = Sayce and Cowley K

<i>Shabat 24</i>	= <i>Athy 9</i>	<i>Year 13/14 of Darius II</i>
February 10	= February 10	410 B.C.
" 29	" "	409
" 18	" 9	408

The following table analyzes the results of our tabulation.

NUMBER OF DAYS BETWEEN BABYLONIAN AND EGYPTIAN DATES

	PAPYRUS NO.							To-TAL	AVER-Age
	1	2*	3	4	5	6	7		
Traditional year....	1	4	4	2	1	1	0	13	1 6/7
First following year..	20	7	15	21	10	10	19	102	14 4/7
Second following year	10	18	4	11	20	21	9	93	13 2/7

\* Taking December 30 as the Julian date.

Unless this analysis can be invalidated—and at present that possibility must be doubted—a most serious objection is here brought out to altering the chronology of Darius I. It is true that we have no double dates from his own time, but our papyri demonstrate that Xerxes' first year fell in 485/84 B.C., and, since we know from the evidence of the Babylonian tablets that Darius ruled thirty-six years, his first year must have been 521/20 B.C.

Now, it is true that the Babylonian dates as given above rest on tables which assume 522 B.C. as the accession year of Darius I, and one might argue that, if one year for Bardiya and one for Nebuchadnezzar III is to be inserted after Cambyses' eighth year, the table would have to be reconstructed from 522 onward. This argument, however, would lead us into considerable difficulties. In order to show this, let us construct a table of the nineteen years of the twelfth cycle after Nabunaisir.<sup>22</sup> This is a normal cycle with seven intercalated

<sup>22</sup> The table is based upon Sidersky, *op. cit.*, p. 63, and *Etude sur la chronologie assyro-babylonienne* (Paris, 1916), pp. 29 and 38. The conventional system of nineteen-year cycles, beginning with Nabunaisir, is here adhered to, although Kugler (*Sternkunde und Stern-dienst in Babel*, II, Part II, Fasc. 2 [1924], 422 ff.) argues for no cycle before 528 B.C., an eight-year cycle from 528 to 505, a twenty-seven-year cycle from 504 to 383, and the nineteen-year cycle from 382 on. This is not the place for an elaborate examination of his argument, which is not completely convincing. The evidence now available supports the theory that the Babylonians, though perhaps without definite formulation, were following a

months, all attested by tablets.<sup>23</sup> There is no period of years longer than three from one intercalated month to another, and there are eight Nisanu 1 dates in March and eleven in April.

Now a most important point is that the Addaru II of the XVIIth year of the cycle is dated not to the eighth year of Cambyses but to

## CYCLE XII

Year of Cycle	Regnal Year	Inter-calated Month	Date of Nisanu 1
I.....	Cyrus 1	.....	March 24, 538 B.C.
II.....	" 2	Ululu	" 12, 537*
III.....	" 3	Addaru	" 30, 536
IV.....	" 4	.....	April 18, 535
V.....	" 5	.....	" 9, 534
VI.....	" 6	Addaru	March 28, 533
VII.....	" 7	.....	April 16, 532
VIII.....	" 8	.....	April 5, 531
IX.....	" 9	Ululu	March 26, 530
X.....	Cambyses 1	.....	April 12, 529
XI.....	" 2	.....	" 1, 528
XII.....	" 3	Ululu	March 21, 527
XIII.....	" 4	.....	April 9, 526
XIV.....	" 5	Addaru	March 29, 525
XV.....	" 6	.....	April 18, 524
XVI.....	" 7	.....	" 7, 523
XVII.....	" 8	Addaru	March 27, 522
XVIII.....	Darius 1	.....	April 14, 521
XIX.....	" 2	.....	" 3, 520

\* This is an unusually low date, perhaps to be explained by the disorganization which must have accompanied Cyrus' conquest.

the accession year of Darius.<sup>24</sup> Hence, the insertion of a year for Bardiya and one for Nebuchadnezzar III, before Darius, would re-

nineteen-year cycle prior to 382 simply on the basis of intercalating a month whenever the calendar required it. One has only to refer to Kugler's table (*op. cit.*, pp. 424-25) to see that from 546/45 to 511/10 there are two nineteen-year cycles which coincide exactly, overlapping all but a small part of his three postulated eight-year cycles. However, even on the basis of eight-year cycles in the period from 528 to 505, it may be categorically stated that an analysis similar to that presented above on the basis of a nineteen-year cycle would result in the same conclusions.

<sup>23</sup> I am indebted to Dr. W. H. Dubberstein for checking this material. The references are given in Sidersky, *Etude*, p. 29, and Kugler, *op. cit.*, Vol. II, 412. The three Ululu's are not unusual in this early cycle.

<sup>24</sup> Three tablets, one of which requires an emendation from Ululu to Addaru, are listed by Poebel, "The Duration of the Reign of Smerdis . . .," *op. cit.*, p. 134.

quire that this Addaru II be lowered to the XIXth year. Thus we should have to correct the above table as follows:

XVII.....	Cambyses 8	.....	March 27, 522
XVIII.....	Bardiya 1	" 16,	521
XIX.....	Nebuchadnezzar 1	Addaru	" 5, 520

At once we can see two important objections: (a) there is a period of five years from one Addaru to another, which, to my knowledge, is not provable in any other cycle; (b) Nisanu 1 is seen to begin March 16 and March 5. Sidersky has tried to demonstrate that Nisanu 1 usually fell on or after the equinox, and only rarely, and by few days, before it.<sup>25</sup> His own later tables do not bear him out completely; but, apart from the exceptional dates of March 11 in the forty-first year of Nebuchadnezzar and March 12 in the second year of Cyrus, the usual dates, when Nisanu 1 falls in March, are in the last third of the month.

Furthermore, besides lowering Nisanu 1 in the XIXth year of the twelfth cycle to March 5, we should also have to lower every date in the following thirteenth cycle (for which we have seven attested intercalated months) by twenty-two days.<sup>26</sup> This would result in the following series:

I	March 23	VIII	March 7	XIV	March 29
II	" 12	IX	" 25	XV	" 18
III	" 1	X	" 14	XVI	" 8
IV	" 20	XI	" 3	XVII	" 26
V	" 8	XII	" 22	XVIII	" 15
VI	" 27	XIII	" 10	XIX	" 5
VII	" 17				

Not one date falls in April, which seems an impossibility for the Babylonian calendar.

But, it may be argued, we need only to intercalate another month somewhere in the five-year period between Addaru's to make everything satisfactory. To this procedure there are three objections: (a)

<sup>25</sup> *Etude*, pp. 70 ff.

<sup>26</sup> It should be stated that we can be quite certain of the Nisanu 1 dates\* or this and the preceding cycle because of an eclipse of the moon which occurred in the seventh year of Cambyses. A record of this has been preserved in Ptolemy's *Almagest* (iv. 14) and in a Babylonian tablet (Strass. *Camb.* No. 400). The latter dates the eclipse to the night of the fourteenth of Duzu, which agrees with the astronomical calculation of July 16, 523 b.c., 39 minutes before midnight (cf. Kugler, *op. cit.*, I, 61 ff.). Accordingly, we must date Nisanu 1 of Cambyses' seventh year to April 7, 523. From this fixed point we can calculate forward and backward in the twelfth and thirteenth cycles, for which we possess certain knowledge of the intercalated months.

in a period when tablets are plentiful we have no evidence for such a month; (b) we should then have a total of eight intercalated months in the twelfth cycle, which a priori is at least most improbable if not impossible;<sup>27</sup> and (c) we should have to raise all the dates of the thirteenth cycle by eight days, giving this series:

I	April 22	VIII	April 6	XIV	April 28
II	" 11	IX	" 24	XV	" 17
III	March 31	X	" 13	XVI	" 7
IV	April 19	XI	" 2	XVII	" 25
V	" 7	XII	" 21	XVIII	" 14
VI	" 26	XIII	" 9	XIX	" 4
VII	" 16				

These dates, with only one in March, seem as improbable as the previous series.

It is unnecessary to go through the same process to check the result of dropping the Addaru II of the XVIIth year by only one place, to the XVIIIth year of the twelfth cycle. The reader can easily see that lowering all the Nisanu 1 dates of the thirteenth cycle by eleven days, or raising them by nineteen, would be equally unsatisfactory.

Our conclusion should be, therefore, that we can neither intercalate another month in the twelfth cycle nor move the Addaru II of year XVII to a later year. Thus we can feel certain of the Julian equivalents of the Babylonian dates in the Aramaic papyri; and, in proving that, we have demonstrated that the Addaru II of Darius' accession year must be placed in the XVIIth year of the twelfth cycle, or 522/21 b.c., and his first year was therefore 521/20 b.c.

## II. THE DATE AND RECONSTRUCTION OF A PAGE OF THE "DAY-BOOK" OF THE MEMPHIS ARSENAL

In 1931 N. Aimé-Giron published a number of Aramaic texts, among which were some papyrus fragments found by Firth at Saqqarah in 1926.<sup>28</sup> Bearing the numbers 5 to 24 in his publication, they are said by him to constitute parts of a "day-book" kept at the Memphis arsenal or dockyard. Although the larger fragments frequently bear parts of dates, both Egyptian and Persian, no king is

<sup>27</sup> It is true that Pohl, *NRu*, I, No. 20, gives an Addaru II to the second year of Nabu-naid, making eight for the eleventh cycle, but this must be either a copyist's or scribal error, as we should then have three Addaru II's in successive years, and four in one five-year period, bringing a Nisanu 1 back to May 7—all quite unlikely. Furthermore, it would be impossible to attribute the extra month of Cycle XII to the following cycle, for we have seven attested months for that cycle.

<sup>28</sup> *Textes araméens d'Egypte* (Le Caire, 1931), pp. 12 ff.

ever mentioned, and the editor demonstrates no date for their composition.

Study of the fragments has led me to the conclusion that they can be dated with certainty to the reign of Xerxes, in a period from October 25, 472 B.C., to May 17, 471. Moreover, at least five of the fragments—Nos. 10, 11, 13, 14, and 15—once formed the larger part of a single page<sup>29</sup> of the "day-book." The method, based upon the fragmentary dates, by which this page was reconstructed need not be detailed here, but the correctness of the reconstruction requires demonstration.

As can be seen in Plate IV, the recto of the page bore two columns, of which approximately half of column A is missing on the right. The verso, Plate V, apparently bore but one column. Four dates are preserved, in whole or in part, and we may tabulate them as follows:

Recto A 9 . . . . .	= . . . [Choiak] year . . . .
B 3 11 (+x) . . .	year 14 = 3 Choiak, year . . . .
B 11 23 Addaru, ye[ar XII]II	= 8 Choiak, year . . . .
Verso C 3 24 Addaru, year [XII]II	= 9 Choiak, year 15

There is no question of year 14 (Persian) being correct in C 3, for whenever two years are mentioned, the Persian is one less than the Egyptian.<sup>30</sup> The sequence of tabulated dates thus conclusively proves our arrangement of the fragments. No doubt, the still large holes might be filled in to some extent with the smaller fragments, but without the physical evidence of the papyrus as a guide that would be a hazardous venture.

We can now restore with certainty Addaru 18, in B 3.<sup>31</sup> Also in A 9 the date must have been Addaru 16 or 17=Choiak 1 or 2. In the reign of what king did Addaru 16, year 14, equal Choiak 1, year 15? Four kings are possible; and we tabulate the results for each, arrived at as explained in Section I, C, of this article.

Addaru 16, year 14	= Choiak 1, year 15
Darius I	March 26 = March 28, 507 B.C.
Xerxes	" 18 = " 19, 471
Artaxerxes I	" 26 = " 14, 450
Darius II	" 21 = " 3, 409

<sup>29</sup> This page could, of course, be merely the end of a long papyrus roll. Elsewhere in this issue (pp. 302 ff.) Dr. R. A. Bowman discusses the restored text.

<sup>30</sup> Cf. Sec. III of this article.

<sup>31</sup> The editor read 17(?), *op. cit.*, p. 32.

Artaxerxes I and Darius II may be immediately eliminated, but at first glance it would seem difficult to decide between Darius I and Xerxes. The following considerations make a decision easier. We are concerned with a governmental archive which has frequent entries—a record which should offer little opportunity for casual errors in double-dating. It is easily possible to account for a lack of agreement by one day, but a two-day interval would require a somewhat Procrustean explanation.

In Xerxes' fourteenth year Tebetu began on January 4, 471, Shabatu on February 2, Addaru on March 3; Nisanu of his fifteenth year began on April 2.<sup>32</sup> This means that both Tebetu and Shabatu were months of twenty-nine days. Now it is extremely doubtful that any other aid than actual observation of the crescent was in use in Egypt for the determination of the month's beginning; and, if the evening of the twenty-ninth day of Shabatu were cloudy at Memphis, it is quite possible that the crescent would not be seen and that the month would be given another day, especially as the preceding month had but twenty-nine days. This would result in Addaru's beginning on March 4, and Addaru 16 would then be March 19, in complete agreement with the Egyptian date of Choiak 1.

Addaru in Darius' fourteenth year began, by astronomical calculation, on March 11. Even if the preceding month had but twenty-nine days, under no circumstances could the first day of Addaru be delayed more than one day. The king of these fragments, then, can be only Xerxes.

We can now date other fragments and list all of them in their chronological order. The earliest is No. 8, and it should be noted that the verso was written before the recto.<sup>33</sup>

8 v. 19 Tash[ritu]	= [11 Epiphi, year 14] = Oct. 25, 472 B.C.
20 Tashritu	= [12 Epiphi, year 14] = Oct. 26, 472
8 r. [27 Tash]ritu	= 19 Epiphi, [year 14] = Nov. 2, 472
5 r. [ 6 Addaru, year 14]	= 21 Athyr, [year 15] = March 9, 471

<sup>32</sup> These dates have all been calculated astronomically by the use of Schoch's new moon tables in Langdon and Fotheringham, *The Venus Tablets of Ammizaduga* (London, 1928).

<sup>33</sup> The assumption is that the editor named as recto the side with the horizontal fibers uppermost. While it was generally true that the Egyptian scribe wrote first on this side, there are numerous examples where the opposite is true. The year must also be 14, not 15, as the editor proposes, because in year 15 of Xerxes, Epiphi 19 could not have fallen in Tashritu.

Now come the dates on the reconstructed page, given in the table above:

A 9 =	March 19 or 20, 471
B 3 =	" 21, 471
B 11 =	" 25, "
C 3 =	" 26 "
18 r. 30 Nisanu <sup>34</sup>	= [14 Tybi, year 15] = May 1, 471
12 r. [x Nisanu or Airu]	= [x Ty]bi, year 15 = April 18-May 17, 471 (Airu begins May 2).

### III. PERSIAN AND EGYPTIAN METHODS OF DATING

Through our discussion of the Aramaic papyri from Egypt we have become aware that there was not complete accord between Egyptian and Persian dating. The names and usually the days of the months differ, and some of the papyri record two regnal years—always, it should be noticed, consecutive. We have now to inquire into the reasons underlying this double dating.

Under the Twenty-sixth Dynasty the regnal year coincided with the civil year,<sup>35</sup> which began with the first day of the first month of the first season, in Persian times and thereafter called Thoth 1. That portion of the civil year which remained after the death of a king was counted as *year 1* of his successor.<sup>36</sup> According to the Persian method of dating adopted from the Babylonians, the regnal and civil years also coincided, beginning with Nisanu 1, but the unexpired part of the civil year after a king's death was called the *accession year* of his successor. It should perhaps be further explained that the Egyptian year consisted of twelve months of thirty days each with five epagomenal days added at the year's end, while the Babylonian year was made up of twelve or thirteen lunar months of twenty-nine or thirty days each.

As we shall demonstrate, the Egyptians in part accommodated their dating to that of their Persian overlords as follows: (1) they kept

<sup>34</sup> Astronomically, Nisanu had but twenty-nine days, a clear indication that *observation* of the crescent was relied upon.

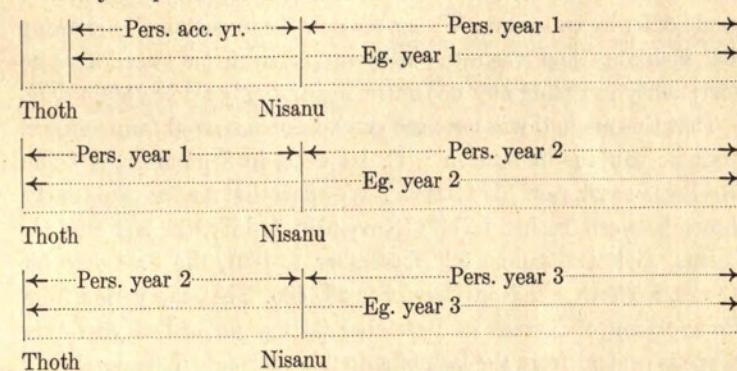
<sup>35</sup> This was also true of the Middle Kingdom. In the Eighteenth Dynasty, however, and probably in the following dynasties, regnal years were counted from the day of accession, independently of the civil year (cf. Eduard Meyer, *Aegyptische Chronologie* [Berlin, 1904], pp. 187 ff.).

<sup>36</sup> Thus, e.g., from the reign of Psammetichus III (only six months according to Manetho and Herodotus) we have a demotic papyrus dated to the month of Tybi, year 2. His first "year" could, therefore, have been at most no longer than one and a fraction months, as Tybi is the fifth month of the year (cf. Spiegelberg, *Dem. Pap. Strassburg*, No. 2, p. 15.).

Thoth 1 as their date for beginning regnal years, but (2) they adopted the principle of the *accession year* whenever the new king came to the throne after Nisanu 1 and before Thoth 1.

The first is easily proved from the double year-dates in the Aramaic papyri.<sup>37</sup> In 525 b.c. Thoth 1 fell on January 2 (Julian). A hundred years later, since the Egyptian year was 365 days and thus lost a day every four years, it came on December 8, 426. Babylonian Nisanu 1, however, always fell near to or shortly after the spring equinox. Thus any given regnal year of a Persian king began in Egypt three to four months before it began in Babylonia. Any Aramaic papyri written in this period would bear two year-dates, with the Egyptian always greater by one than the Persian. After Nisanu 1 the years would again coincide and be written only once.

The demonstration of the second proposition rests on the fact that such double year-dates are not found except in the three to four months' stretch of the Egyptian year prior to Nisanu 1, and that these double dates are always of consecutive numbers. To clarify this point, let us illustrate the usual Twenty-sixth Dynasty dating for a Persian king who came to the throne after Thoth 1 but before Nisanu 1. Over a three-year period we would have the following:



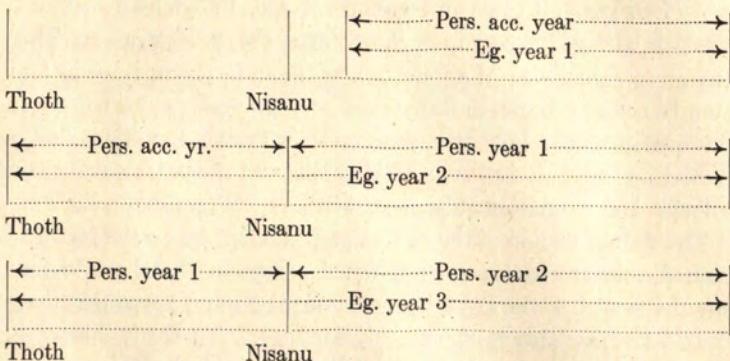
The accession of Darius II would fit such a dating situation, for he came to the throne in early February, 423 b.c.<sup>38</sup> Thoth 1 came on

<sup>37</sup> No double dates for this period have yet been noted in any document written in Egyptian.

<sup>38</sup> The last recorded date for Artaxerxes I is Shabatu 17, February 26, 423 (BE, IX, No. 109), while the first for Darius II is Shabatu 2, February 15, 423 (BE, X, No. 1). I owe the information for this and the following footnote to Dr. W. H. Dubberstein.

December 7, 424, and Nisanu 1 on April 11, 423. Correctly, therefore, papyri Nos. 6 and 7 in Section I, C, above give double year-dates for Thoth 12 and Athyr 9.

Now let us assume that the usual dating was also followed for a king who came to the throne after Nisanu 1 and before the next Thoth 1. Our diagram would then be this:



It is at once clear that the two regnal years would never coincide and that there would be a three- to four-month period yearly in which they would differ by two years. We can see now the importance of knowing that such a method was never in operation, so that we need have no uncertainty in dating any document bearing only an Egyptian date.

That this method was not used can be demonstrated from our first Aramaic papyrus in Section I, C. Its date, in September, is Ululu 18 = Pachons 28, year 15 of Xerxes. We know that Xerxes came to the throne between Tashritu 27(?) (November 7, 486), the last date for Darius, and Arahsamnu 22 (December 1, 486), the first date for Xerxes.<sup>39</sup> Thoth 1 fell on December 23, 486. That the period from the accession of Xerxes to December 23 was not called year 1 in Egypt is evident from the lack of a double year-date in September of his fifteenth year. Confirmation is obtained from II above, where a date of year 14/15 in Addaru/Choiak, after Thoth and before Nisanu, can fit only Xerxes.

It cannot be argued that the news of the accession of Xerxes might not reach Egypt until after Thoth 1 and that the circumstances,

<sup>39</sup> VAS, IV, No. 180; V, No. 117.

therefore, would be the same as those of Darius II. Even though the news might be late, the date of accession would certainly be known, and Egyptian dating would be based on it. It seems quite clear, therefore, that the Egyptians consciously adjusted their dating to avoid any such situation as outlined in the second diagram above.

While we are thus sure of the accuracy of dates from Darius I<sup>40</sup> on, there remains some uncertainty for Cambyses, arising from the fact that two dating methods came into use after the conquest. According to the first, the six-month reign of Psammetichus III was disregarded and Cambyses' year 1 was considered the balance of the civil year after the death of Amasis (526 B.C.). In his second year, therefore, Cambyses actually conquered Egypt, and in his fifth year he died.<sup>41</sup>

In the second method employed the Persian regnal years were taken over and applied retroactively to Egypt, wiping out not only the reign of Psammetichus III but also the last four years of Amasis.<sup>42</sup> It can be seen, then, that any date falling in years 1–5 of Cambyses might be off three years, and other data must be used to determine it accurately.

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<sup>40</sup> We have no double-dated papyri from his reign, but the Apis stela discussed in Sec. I, A, gives us assurance. Had the balance of Cambyses' eighth year been counted as the first of Darius, the life of Apis could have been only six years, and there is no justification at all for reading the disputed numeral as 6.

<sup>41</sup> This dating is found in *Ryl. Dem. Pap.*, No. IX, written in the ninth year of Darius (cf. Griffith, *Ryl.*, III, 106).

<sup>42</sup> Cf. *Cairo Dem. Pap.*, No. 50059 (Spiegelberg, *Catalogue général ... du Caire. Die Demotischen Denkmäler*, III, 42 ff.), which refers to year 2 and year 8 of Cambyses.

## PLATE IV

## AN ARAMAIC JOURNAL PAGE

RAYMOND A. BOWMAN

At the request of Richard A. Parker, who has reconstructed and dated a page of papyrus from several fragments of a series which Aimé-Giron had already suggested must somehow belong together as a journal of the Memphis arsenal,<sup>1</sup> I have prepared this reading of the reconstructed text (Pls. IV-V).

These badly damaged fragments of text are somewhat unique in having their most legible readings in the date formulas which epigraphers from experience usually expect to find most difficult. Aside from the date lines, the text is in very poor condition. Portions are so mutilated that only isolated letters or even traces of letters are discernible, and gaping holes make it almost impossible to gain an intelligible context.

Under such circumstances it is difficult to improve on the usual careful work of Aimé-Giron and almost impossible to glean more than he has from them. However, the demonstration that the pieces can be read when the fragments are put together as Parker has done it, by matching lines and by joining the legible date formulas, can be regarded as a contribution in advance of what Aimé-Giron has done. Then, too, my independent readings of the photographs of these fragments sometimes differ from those already proposed. Often, it is true, these differences depend upon an alternative interpretation of very mutilated letters, but these readings might throw light on some of the more legible sections of text and, if sound, might affect Aramaic lexicography.

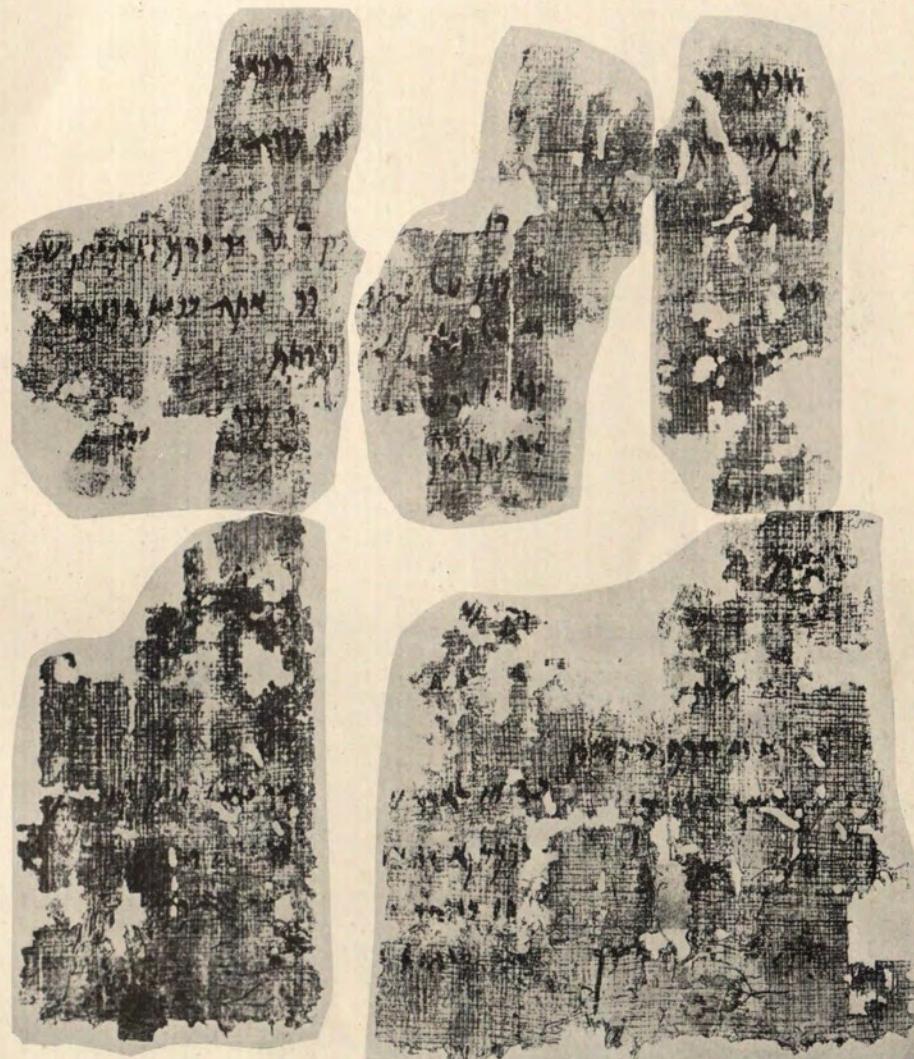
TEXT<sup>2</sup>

## COLUMN A, RECTO

ן כוותה פר[מו]נבריא .....	(1)
ן אביוו עלה ס[ו]ב[כ] .....	(2)

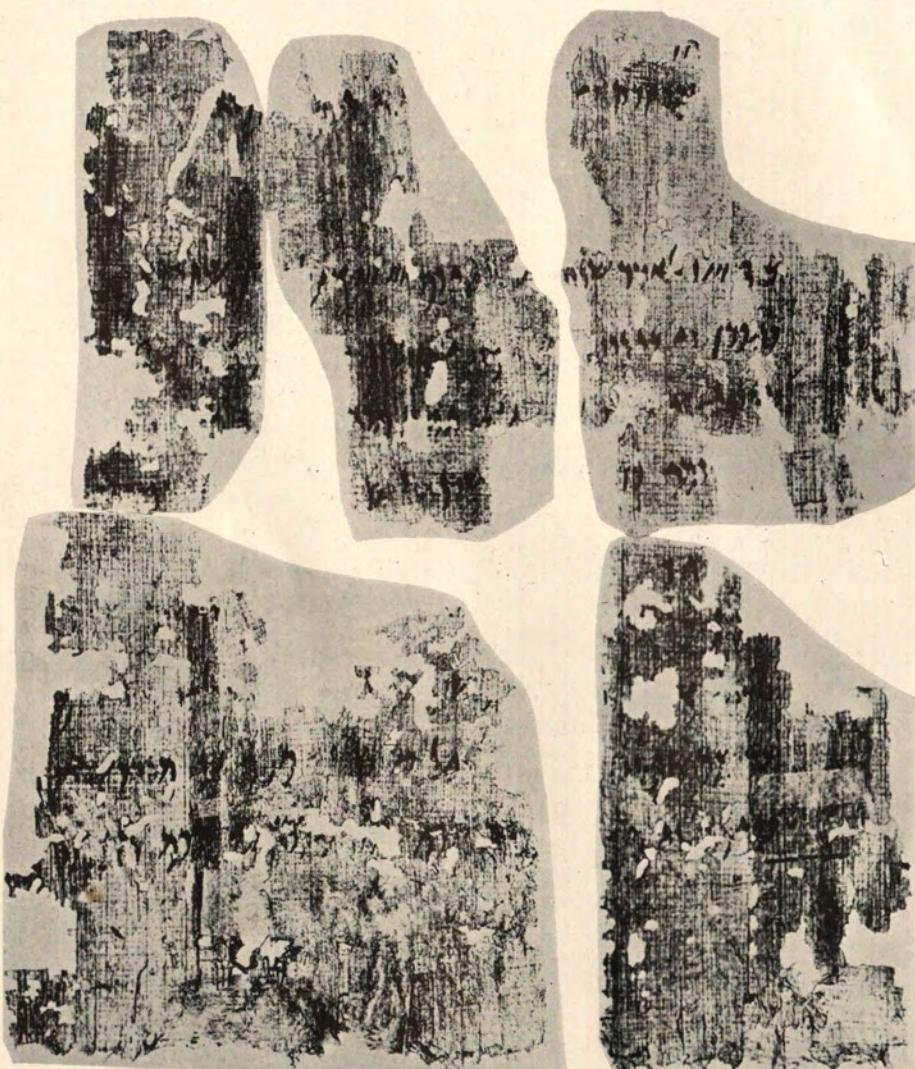
<sup>1</sup> Cf. p. 295 above. N. Aimé-Giron, *Textes araméens d'Egypte* (Cairo, 1931), pp. 12 ff. (hereafter abbreviated "A-G").

<sup>2</sup> I attempt to indicate as closely as possible the approximate state of the text. Letters about which there is some doubt are dotted, those even more doubtful are dotted in



AN ARAMAIC JOURNAL PAGE

PLATE V



AN ARAMAIC JOURNAL PAGE

בְּנָה ..... (3)  
בְּגַפְרִן ... יְדָה ..... (4)  
סָפִינָה I ذֵת חֲנָה ..... (5)

יְאָכָל II ..... (6)  
כְּדָר שִׁיטָם טִים ]. ..... (7)  
]○○○○○○○ ..... (8)  
[—]  
בְּ לֹא דָר שָׁנָה X III I הוּא יְום ... לְכִיחָד שָׁנָה [X IX  
תְּ מֶלֶא ذֵת קְדֻם בְּנָה III שְׁבָט ..... (10)  
מְצִירָן יְעַבְּרוּ ..... (11)  
בְּל ... רָאָה ... הַלְּ ..... (12)  
הַוּ יְום מִיIII II לְכִיחָד ..... (13)

COLUMN B, RECTO

אָ ] לִיד חַרְמָה ] בָּר ..... (1)  
בְּ ] לִיד פָּחָה בָּר ..... (2)  
בְּ IX III III II לֹא דָר שָׁנָה X [II III I הוּא יְום III לְכִיחָד שָׁנָה [ II III X ..... (3)  
לְבָרָן עַל בְּגַפְרִן בְּ ] פָּר אַתָּה בְּבֵית סָפִינָה לְמַמְלָא ..... (4)  
דֵי עַל חַבְלָא ..... (5)  
צְלָצָל נֹשֶׁה ] ○○○○○ בָּה ..... (6)  
בְּנָה . תְּנָה . יְדָה I ..... (7)  
כָּל III נִינְדָּה ..... (7)

לְ ] ○○○○○ ] ..... (8)  
בְּ ] ○○○○○ ] ..... (9)  
אָ ] ○○○○○ ] ..... (10)  
[—]

brackets; traces of letters are indicated by the mark ○. Conjectural restorations are un-dotted but are in brackets.

Dots are used to indicate the approximate number of spaces or letters of the text where none remain, where such spaces can be computed at the beginning of or within lines.

A thin vertical stroke interrupts the text in each line where fragments are divided.

(1) ב XX III לאדר ט' נח X III I דר יומן (מ' III II לכידך ט' נח X III III

(12) [אַפְכָרְתָא זִי עֲבֹרוֹ ..... נֵל אָגָוּוּוּ וְסַבְּהַתָּא]  
 (13) [כְּ[תָן בְּהַלְלָא] ..... קָ[נָא] [בְּמַבָּא]]  
 (14) [דִּיגְמָת] ..... [וְ...]

**COLUMN C, VERSO**

(1) ... הָרָה .....  
(2) ... עֲבֹדֶצְקַן דִּי[[עַל ..... [יא ס ...] מֵיָּא כָּא  
[——]  
(3) ב' XX III I לאדר שנה[X] II דר ים III III III לכיה[ח] שנה X  
 פ' II

(4) בְּגַפְרִין זֶה עַל נוֹזֵעַ ... בָּלֶן פֶּה שְׁחַרְבּוֹן וְ... כֶּן בְּבָרֶךְ  
 (5) צְלַצְלָלָה זֶה וְ... אַבְּלָה לְחַמְּשָׁה מְרָאָה וְ... הַלְּ ...  
 (6) וּמְרִיבָּרָה ... שְׂרָן וְ... שְׂרָן ... וְ... שְׂרָן  
 (7) הַר ... |  
 (8) מְנֻן עַל בְּנֵית ... וְ... שְׂפָר [שְׁבָר ...]  
 (9) לְהַגְּזָן אֲבָבָן ... וְ... שְׂמָכָן ... וְ... לְאָזָן ... הַבְּבָם  
 (10) בְּרִיר כְּמַשְׁפְּלָטָה ... אֲלַהֲגָל רַוְּזָה זֶה מְמַפְּרִי] עם מְרוֹכְנָשָׁרָא[...]  
 (11) בְּבַסְסָא זֶה סְלָלָן ... קְדָם וְ... רְדָא [זֶה] קְדָם ...

## TRANSLATION

**COLUMN A**

1. .... his companions the supply officers ....
  2. .... thus they said, "Simak(?) went up (the river?) 3 (days ago?)
  3. .... within
  4. .... Bagaphernes .... hand of(?)
  5. .... 1 boat which is h[ere]
  6. .... the ..... total 1 (plus?)
  7. .... [when] a decree [was made] ....
  8. .... (only traces)
  9. [On the .... of Addaru, year 14, that is the .... day of Choiak] year [15]
  10. .... the ..... which preceded amidst 3 ....
  11. .... Egyptians will make
  12. .... (only scattered letters)
  13. .... [that is] day 8 (or 9?) [of Choiak]

**COLUMN B**

1. .... through the agency of(?) Ḥormaḥi
  2. .... through the agency of Paḥah son of ....
  3. On the 18th day of Addaru, year 14, that is, the 3d day of Choiak, year [15]
  4. for a memorandum concerning Bagaphernes son of .... He went into the boathouse to [inspect a boat?]
  5. which was on the dry land .... Pa-niptēm
  6. a ship(?) ....
  - 6(bis) ....
  7. Total 3 keels(?)
  8. .... [Meshu]llam who ....
  9. [according to] the instruction of Sh .... Shelem[iah] who ....
  10. .... (scattered letters)
  11. On the 23d of Addaru, ye[ar 14], that is the 8th day of Choiak ye[ar 15] ....
  12. the substructure(?) which they m[ade] .... the .SBYT ...
  13. .... (scattered letters only)
  14. .... ....

COLUMN C

1. .... one(?) ....
  2. .... Abdzedeq who is [in charge of] the ..... the water(?)  
.....
  3. On the 24th of Addaru, year 14, that is the 9th day of Choiak, year 15
  4. Bagaphernes who is in charge of cordage (or sails?) ..... Shethra-buzana(?) .....
  5. the boat(?) which they ..... [our?] lord ..... who(?) is in charge of(?) .....
  6. ..... son of .....
  7. ..... he(?) .....
  8. ..... from(?) over the hou[se of the boats?] .....
  9. Belonging to the company of .....
  10. son of Chemosh-pelet the ..... , belonging to the company of Vayaz-data(?) of Memphis, along with Marduk-şar-usur .....
  11. in the wooden bowl(?) which ..... before ..... the ..... [which is before .....

### NOTES<sup>3</sup>

**COLUMN A**

*Line 1 (A-G, No. 14, recto, l. 1)*

The length of the line is determined by line 9 of this column which is assumed to be a new date entry. The number of spaces available is approximated by counting line 3 of column C (the verso), the most complete date line.

<sup>3</sup> The system of transliteration used is that proposed by A. A. Brux, "A Simplified System of Hebrew-English and Aramaic-English Transliteration," *AJSL*, LVIII (Jan-

The conjunction *w* supplied by A-G before *knuth* is not found in the photograph, and there is no room for it. The first traces of a letter show, by its extended staff, that it is *t* or *k* and not *w*. The last word, restored by A-G on the basis of occurrences in the shipbuilding papyrus of 412 B.C.,<sup>4</sup> is convincing, but I have translated as "supply officers" rather than "commanders" because their function in the shipbuilding papyrus was that of ordnance officials who inspected equipment and ordered repairs.

*Line 2 (No. 14, r., l. 2, and No. 15, r., l. a1)*

Instead of the *lh* of A-G we must read *clh* (for there are definite traces of *c* and the space between words would be too great without it), which could be only the verb "he went up," although it is used nowhere else in the papyri. The rest of the line is very obscure. One expects a proper name after the verb. The first darkened traces suggest the head of *s* with a short staff.<sup>5</sup> The next letter is a small one without a staff, probably *y*. This is followed by a definite *m* and the bottom of a long thin staff which almost intrudes into the line below. A-G read it as *n*, which cannot be entirely excluded, but the traces favor a *k*. A name fitting the traces here, *symk*, appears in a papyrus of 418 B.C.<sup>6</sup> Smudges that follow are difficult to read. The last blur has been identified as *z* but the thick parallel strokes suggest the figure 2 or possibly 3. The meaning of such a cryptic figure is not clear. It may be a reference to a journey up the river on the "third" day of the month.

*Line 3 (No. 14, r., l. 3, and No. 15, r., l. a2) and line 4 (No. 14, r., l. 4)*

Obscure traces in line 3, but only the last word is clear. Only one word is clear in line 4, but uncertain traces suggest that the line was once completed.

*Line 5 (No. 14, r., l. 5)*

A-G reads only *spynh* . . . . , which seems certain although the first letter is almost completely lost. A single thick stroke follows this word, then a trace of *z*, a complete *y*, and finally what seems to be *t*. The single stroke is apparently a figure. The practice of writing the numeral for the indefinite article is illustrated by Cowley.<sup>7</sup>

The space following this line is blank for a single line. Since the text of this column preserves only the ends of lines, one cannot determine whether this blank is merely the end of a short line or a deliberate dividing space.

*Lines 6 (No. 14, r., l. 7), 7 (No. 10, r., l. 1), and 8 (No. 10, r., l. 2)*

uary, 1941), 57 ff. In order to reproduce the unpointed text, however, each character, consonant or vowel, is written in the consonantal value.

<sup>4</sup> A. E. Cowley, *Aramaic Papyri of the Fifth Century B.C.* (Oxford, 1923), No. 26, ll. 4 and 8.

<sup>5</sup> Cowley, *AP*, No. 5 (471 B.C.), 1. 16. Cf. the *s* in the name *m̄sh* in the plate for this papyrus in A. H. Sayce and A. E. Cowley, *Aramaic Papyri Discovered at Assuan* (London, 1906), Papyrus A, Pl. 2, 1. 16.

<sup>6</sup> Cowley, *AP*, No. 22, 1. 27.

<sup>7</sup> *Ibid.*, No. 5, l. 4 (471 B.C.) and elsewhere.

*Line 9 (No. 10, r., l. 3)*

Apparently a date line introducing a new entry, as in lines 3 and 14 of column B and line 3 of column C. If this be so, one may then approximate the width of this column by reference to those more complete lines.

*Line 10 (No. 10, r., l. 4)*

A-G reads . . . . . *ml<sup>z</sup> zy qdm bywm-št*, which he understands to say *ml<sup>z</sup>* which "arrived" in the "sixth day." As far as *zy*, the line is very badly damaged. On the edge are traces of what might be *t*. The *l* and *z* are rather certain. The second letter of *qdm* looks like *w* but may be a poor *d*. As indicated above, this is regarded as a verb by A-G, who says: "Qdm doit être considéré ici comme un verbe, probablement au *pael* avec le sens de 'se présenter, arriver.'" *Qdm* does occur in the papyri as a verb (in Cowley, *op. cit.*, No. 82, l. 6) in the sense "to be presented" and in the Targums in the sense "to precede," "to be early."

Immediately after *qdm* the letters get smaller and crowded, as though attempting to avoid the next column. It would seem that the smaller letters are a later addition to the text, like the interlinear letters in B6 bis.

It is impossible to read *bywm* here, for the second letter is definitely *g*, and what would have to be read *m* would have no counterpart in the papyri.

The *št* read by A-G seems possible but would be unusual for a numeral here where strokes are ordinarily used. The best reading is *šbn*, although *šnn* with the second *n* short and curved cannot be excluded.

*Line 11 (No. 10, r., l. 5)*

A-G reads . . . . . *bm̄syn y<sup>b</sup>bdun*. The *m* is quite probable, but the *b*, read as clear by A-G, cannot be identified. The reading *y<sup>b</sup>bdun* is certainly preferable to his alternative reading *y<sup>b</sup>qun*.

*Line 12 (No. 10, r., l. 6)*

I have been tempted to read *bbbl*, "in Babylon," but the second *b* is improbable. What has been read as *h* might also be *b*, which seems to be followed by traces of two more letters of which only the tip of *l* suggests anything. A-G ignores these last traces.

*Line 13 (No. 10, r., l. 7)*

A-G reads . . . . . *p<sub>o</sub>m* . . . . . *[k]yhhk*. Since it is probably correct to read the month name at the end of this line, one may look for other signs of a date formula. This I see in *ywm*, "day," as in column B, line 3. If this be correct, the space before the month name is enough for eight or nine strokes.

COLUMN B

*Line 1 (No. 15, r., col. B, l. 1, and No. 11, r., l. 1)*

A-G reads the *z* as clear, but there is scarcely enough of a trace to identify the letter. For *lyd* we may read "through the agency of —" parallel in meaning to the common expression *ana qāti* of the Neo-Babylonian business docu-

ments. The proper name has been identified as the Egyptian *Hr-m-ḥ-t*, with the final element written *mḥy*, as in the Aramaean spelling, *Whpr-mḥy*.

*Line 2 (No. 15, r., col. B, l. 2, and No. 11, r., l. 2)*

The name *Pḥḥ* is identified as the Egyptian *P-ḥ̣t* which is found in demotic as *P-a-h<sup>e</sup>* and in Greek as *paēs*.<sup>8</sup>

A horizontal check line just below the first letter of this line and extending into the space between the columns marks the end of an entry and is followed by a space a little wider than normal between lines in this papyrus.

*Line 3 (No. 15, r., col. B, l. 3, and No. 11, r., l. 3) and line 4 (No. 15, r., col. B, l. 4, and No. 11, r., l. 4)*

A-G does not associate these two fragments. Number 15, recto, column B, line 4, he reads as above, but the second, he believes, starts with *m*, the last letter of a personal name followed by *br*, "son of," with the *yth* as the second name. However, what has been read as the *b* in *br* looks much more like *p* or even *w*; since it has no definite head, the traces of the letter before this word in the photograph look more like *r* than like *m*. It is imperative from the join on the verso that there be a bit of space between the two fragments, if Parker's restoration of the date is certain. This would permit the reading of the *n* of Bagaphernes and the *b* of "son of" in the space and regarding the *pr.* or *wr.*, whatever it may be, as a second name. The *yth* is in this context more likely to be a verb than a personal name.

The *lm* at the end would seem to be not an adverbial particle indicating the beginning of the message but the first part of an infinitive form.

*Line 5 (No. 15, r., col. B, l. 5, and No. 11, r., l. 5)*

Again appears the troublesome *l tbl* first encountered in the shipbuilding papyrus (*AP*, No. 26, ll. 4 and 8). A-G cautiously makes a suggestion that *tbl* may be the Hebrew and Aramaic *tēbhēl*, "world," in the sense of "dry land." Assyrian *tābālu* often occurs in contexts where "dry land" is contrasted with "water." A-G thinks of a "dry dock" in this place but feels that this meaning does not fit in Cowley's context.

It does not suit the context as Cowley's restorations have constructed it, but some of the attempts at restoration are definitely wrong in that difficult papyrus. Cowley's own rendering of *l tbl* as "according to measure, i.e., accurately" compared with Hebrew *ḥeabhal*, "measuring line," is as poor as his *ytnqd*, "drawn up," in the sense of "specifications." Against the idea that the "boat" be "drawn up," Cowley raised two objections: first, that the word for boat is always the feminine *spynh* and would not therefore be used with the masculine verb *ytnqd* in *AP*, No. 26, line 4, and, second, that whatever was "drawn up" had to be sent to the treasury.<sup>9</sup> Consequently, in his restoration of line 3, he supplies *ṣrn*, understood as "specifications," as the

<sup>8</sup> Cf. W. Spiegelberg, "Ägyptisches Sprachgut in den aus ägypten stammenden aramäischen Urkunden der Perserzeit," *Orientalische Studien Theodor Nöldeke* (Gieszen, 1906), II, 1103, No. 24.

<sup>9</sup> *AP*, p. 92, sub. 1. 4.

object to be "drawn up accurately." But the subject need be neither "boat" nor "specifications." It might be a part of a boat that is regarded as of masculine gender. One could suggest "keel" as something that might be "drawn up" on land. It is possible that the *ngydh* of *AP*, No. 26, line 8, is to be read "its keel" or "its prow." The root *nqd*, with the meanings "stretch, draw, pull, prolong, guide," would be an excellent one from which to derive a noun "keel" or "prow," since several of the meanings would fit the requirements for a long projecting part of a ship that was of some use in dragging it up on the beach. I suggest that *ngydh*, "its keel," be substituted for Cowley's *ṣrn* in the reconstruction of line 3 and read also in line 8. This would overcome the first of his objections.

As for the second objection, if the text is properly read, nothing is said about sending a boat or specifications to the treasury. As elsewhere in the shipbuilding papyrus (cf. l. 6) the verb *slh* in line 4 means "send word"; the treasury officials were to be notified when the boat was ready for inspection.

If further need exists to support A-G's interpretation, it is found in line 7 of the shipbuilding papyrus (*AP*, No. 26), where it is definitely stated that the boat was "[upon] the sand which was before the fortress," just where the [*ngydh*] *ytnqd* [l *tbl*] of lines 3 and 4 instructed the boat should be. In my opinion, with its proper context, the meaning of *l tbl* as "dry dock" fits the context of the shipbuilding papyrus.

After broken context the last clear word in the line seems to read *pnptm*, which A-G regards as possibly an Egyptian name formed by prefixing the element *p* to the name *Nfr-Jtm*. But he rightly notes that one would then expect the spelling *np̄tm* written with an *ṇ*. More probable is the suggestion made to me by Parker that the god-element is *Nfrtm*, the son of Ptah, the principal god at Memphis. Such a formation as *P-Nfrtm* would be appropriate for a local Egyptian. It has already been recognized that the element *nfr* can be spelled without the *r* in personal names, as in *Pt-Nphtp*. But it is interesting to find that the name *Nfr-tm* is written in Babylonian as *-ni-ip-te-e-mu*.<sup>10</sup> Since our name is spelled in Babylonian fashion and the spelling of the names *Marduk-ṣar-uṣur* and *Memphis* (col. C, l. 10) likewise show Babylonian spelling, it seems quite likely that our scribe had Babylonian rather than Egyptian training.

The remainder of this line is blank.

*Line 6 (No. 15, r., col. B, l. 6, and No. 11, r., l. 6)*

The third letter of the first word is very difficult, but the traces suggest nothing better than A-G's *s*, peculiarly made but proved by the example in the name *bdṣdq* in column C, line 2. Apparently the same word appears as *slṣl* in line 5 of column C, with which A-G cautiously compares the *silṣal knāphāyim* of Isa. 18:1, which is rendered by the Septuagint as *ploīōn pteruges*. Thinking of the verb *ns* with the traces that follow, he renders this as "bateau de charge."

<sup>10</sup> H. Ranke, *Keilschriftliches Material zur altägyptischen Vokalisation* (Berlin, 1910) p. 40.

Where I read . . . *w bwh* . . . . . , A-G has "kwgoō." He suggests that this and the following line contain proper names, but there is too little evidence to venture even a conjecture here.

*Line 6 bis (No. 15, r., col. B, l. 6 bis, and No. 11, r., l. 7)*

An interlinear line in smaller letters, apparently a later addition. Whether the last line on fragment 11, recto, belongs to this line or to the line below is not easily determined. The next line, as a total, was probably short. This leads one to believe that the interlinear line was begun with small letters, but, when the line below concluded, the letters became larger as the interlinear line was finished.

In contrast to my reading, A-G has in fragment 15, *byooot hzh* . . . . . and in fragment 11, line 7, recto, . . . . . *yd I*. A third possibility for beginning the line is *b . . . k I znh* . . . , but this is less likely, for there are traces of another letter immediately after what would then be *znh*.

*Line 7 (No. 15, r., col. B, l. 7)*

As A-G has noted, the things or persons here enumerated can be read either *ngyd* or *ngyr*. The occurrence of *ngyd* in this context would be another witness to the possibility of a substantive in the *ngydh* of line 8 of the shipbuilding papyrus.

One would scarcely read *nāghidh*, "leader" or "ruler," here. If the preceding entries are concerned with boats, as they seem to be, and with repairs, as *cltbl* would signify (if read as "dry dock"), then one might well read "keels" here as the type of repairs made.

Immediately after the total there is a horizontal line, as after line 2, and a space marking the end of an entry.

*Line 8 (No. 13, r., l. 1)*

A-G reads on this darkened fragment . . . . . *om* . . . *zy* . . . . . The beginning of the line, which should be on fragment 10, recto, as column B, line 1, is entirely gone, and, as the photograph shows, little can be done with the next two lines also.

*Line 9 (No. 10, r., col. B, l. 1, and No. 13, r., l. 2)*

This line is much destroyed. It has been read as *ooit š* . . . . . (in frag. 10) and . . . . . *šlm oo[z]y ng* . . . (in frag. 13), but I have indicated here only what I can see in the photograph. The *[k]pm* is probable; it is the term used in the papyri to indicate one dictating to a scribe<sup>11</sup> but might also be used to indicate one giving orders.

*Line 10 (No. 10, r., col. B, l. 2, and No. 13, r., l. 3)*

A-G reads in No. 10 *ooo* . . . *tin y* . . . . . , and in No. 13 he questions whether there is any writing at all. The end of the line is probably blank, indicating a short line ending an entry. There is no horizontal line, since that part of the papyrus is destroyed, but a blank line is left after the entry.

*Line 11 (No. 10, r., col. B, l. 3, and No. 13, r., l. 5)*

The use of brackets in the transcription by A-G does not accord with what is visible in the text.

<sup>11</sup> Cf. Cowley, AP, No. 5, l. 15; No. 6, l. 17; No. 8, l. 28, etc.

*Line 12 (No. 10, r., col. B, l. 3, and No. 13, r., l. 6)*

The first word is a probable restoration based on an occurrence of the word in the shipbuilding papyrus where it is also associated, as here, with the verb "to make."<sup>12</sup> From the traces that remain this is a better restoration than the second one that A-G offers, [n]wpkr̩, "construction de bateau," derived from Persian *navi* and *krt*. Although this word must still be classed as "unknown," it is probably derived from the Persian *upa*, "under,"<sup>13</sup> and *kar*, "make," and, if not part of a ship, is probably a reference to a substructure upon which a boat will be set while being built or repaired. In the shipbuilding papyrus the ordnance officials who inspect the boat in need of repair are to make the *>wpkr̩*. Since there (ll. 8-9) the superintendent of the carpenters is presumably included among the ordnance officials, it was doubtless he who had charge of making the *>wpkr̩*.

For fragment 13 A-G reads . . . . . *l̩:l̩:n̩ wpoyt̩*, but the papyrus is too destroyed by wear and stain to be certain of any reading. The *sbyt* I read suggests at once the *spytkn* of the shipbuilding papyrus (l. 9) which Cowley understood as a place name (cf. also *spt*, "name"), the home of the chief of the carpenters. But the second letter, although somewhat doubtful, looks more like *b* than *p*.

*Line 13 (No. 10, r., col. B, l. 5, and No. 13, r., l. 6)*

A-G has here *kotn bhoo* . . . . . , with all the letters marked as doubtful.

*Line 14 (No. 10, r., col. B, l. 6, and No. 13, r., bottom)*

This has been read "oo<sub>o</sub>kygzt," which A-G suggests may be the Persian name Kaigazat(?) formed with the element *azata*, "noble." The first letter might be *d* or *r* but hardly *k*.

Nothing is legible in this line on fragment 13. There once may have been a few letters, now blurred, at the beginning. Apparently it was a short line, ending just beyond the edge of the break.

COLUMN C

*Line 1 (No. 11, verso, l. 1) and line 2 (No. 11, v., l. 2, No. 15, v., l. 1, and No. 14, v., l. 1)*

A space two lines deep follows this entry.

*Line 3 (No. 11, v., l. 3, No. 15, v., l. 2, and No. 14, v., l. 2) and line 4 (No. 11, v., l. 4, No. 15, v., l. 3, and No. 14, v., l. 3)*

Perhaps this Bagaphernes is the same as in column B, line 4, and possibly is to be read in column A, line 4, also. In the papyri this name is found for a judge (*dyn*) who "came up (the river)" to Syene in the thirty-first year of

<sup>12</sup> Cowley, AP, No. 26, l. 5.

<sup>13</sup> This element is found in Sanskrit in the sense of "under" and also in Greek as *τρο* but is not found in Old Persian. However, in the Aramaic from Persepolis alongside the word *gnzbr̩*, "treasurer" or "stores keeper," is found *>pgnzbr̩*, which W. Eilers has suggested must be "subtreasurer." This would indicate that the element *>wp* here may mean *sub-* and that other words, such as *>wpśr*, beginning with this element, are to be explained in the same fashion.

Artaxerxes I (435 B.C.); and as the son of *Wšhy*, it is mentioned as that of a contributor to the fund of the Jewish temple at Elephantine in 419 B.C.<sup>14</sup>

A-G has translated *nuzyn* as "cables" or "voiles," deriving the terms from the root *nuz*, meaning "to twist, twine; to weave."

For fragment 15 the reading by A-G is . . . . . *oob yštnrwn*, of which he translates only "qu'ils se gardent," as though from the root *šmr*. But there is no trace of the initial *y*, and the *m* would be very peculiar. I prefer to read it as the Persian name *Šēthrabūzana* (Old Persian *Xšāthrabūjyāna*) found elsewhere in the papyri as *Štbrzn*<sup>15</sup> and in the Aramaic rescripts from the time of Darius as *Štharbōzney*.<sup>16</sup> This reading, more in accordance with the Persian pronunciation than the others, satisfies all the traces of letters, although the *r* is crowded. For the uncertain traces on fragment 14 only the *b* has been suggested as legible heretofore, and even that has been marked as doubtful.

*Line 5 (No. 11, v., l. 5, No. 15, v., l. 4, and No. 14, v., l. 4)*

For the *slsl* compare the note on column B, line 6 above. In the first fragment A-G's readings end with the *zy*. In fragment 15 he reads . . . . . *oob th̄n mr* . . . . . For the last he reads only . . . . . *y š* . . . . .

*Line 6 (No. 11, v., l. 6, No. 15, v., l. 5, and No. 14, v., l. 5)*

A-G has read for fragment 15 . . . . . *šrno zooo* . . . . . and for number 14 merely traces with nothing legible.

*Line 7 (No. 13, v., l. 1)*

This has been read [ . . . . . *b]gw*] by A-G, but the *h* seems quite clear with no trace of a preceding *b*. A slanting trace of a tail of a letter seems to be *p*, because of its length below the line, but it might be either *r* or *d*.

*Line 8 (No. 13, v., l. 2, and No. 10, v., l. 1)*

For fragment 13 . . . . . *mn . . . cl ooo* has been read heretofore and for 10 . . . . . *wn . . . š d . . . p . . . .*

*Line 9 (No. 13, v., l. 3, and No. 10, v., l. 2)*

Since the *dgl* referred to is presumably at Memphis, it is difficult to determine the name of its commander. At Elephantine names of commandants beginning with *š* are the Persian *rtbnw* and *truprn* and the Babylonian *dnnbw*.

For fragment 10 A-G reads "ošo . . . oo . . . l ol̄n oooot . . ."—too broken to permit even a conjecture for restoration.

After this line he notes a vacant space, but, according to the photograph, there is nothing unusual in the spacing here.

<sup>14</sup> Cowley, *AP*, No. 16, II, [1] and 6; No. 22, I, 133.

<sup>15</sup> Cowley, *AP*, No. 5 (471 B.C.), I, 16, and possibly to be restored in No. 13 (447 B.C.) 1.18.

<sup>16</sup> Ezra 5:3, 6; 6:6, 13.

*Line 10 (No. 13, v., l. 4, and No. 10, v., l. 3)*

The Moabite name *kmšpli* is unique. A-G cites the divine name *kmš* used in the Moabite stone and the use of the element *-pli* in Hebrew names<sup>17</sup> but does not note personal names using the Chemosh element. It is interesting to note that, aside from the name *Chemosh-nadbi* found in the annals of Sennacherib,<sup>18</sup> other examples in cuneiform are found, like *Chemosh-pelet*, in the Persian period. In the sixth year of Cambyses a business document from Babylon involves the "Babylonians" *Itti-nâbu-balâtu* and *Sin-kitri*, both sons of one bearing the hybrid name *Chemosh-šar-ušur*.<sup>19</sup> Among the witnesses in a business document involving prominent and official individuals, dated to the tenth day of the second Addaru of the sixteenth year of Darius (computed by Parker to be March 27, 505 B.C.), we find mentioned Ḥanṭušu the son of Chemosh-ilu.<sup>20</sup> It is apparent that from the beginning of the Persian period Moabites participated in the activities of the Persian empire, often in prominent positions, as far away from Moab as Babylonia and Egypt.

In the translation I have tried to indicate that some official title must have followed the personal name.

The name of the commandant of the company is read by A-G as *Wyoot*, but there is scarcely room for two letters in the gap. Since *z* takes up but small space, my conjecture may be correct.

A-G's reading *mnpv* for Memphis, while in agreement with the spelling of the name elsewhere in the papyri,<sup>21</sup> does not agree with the somewhat obscured traces here. The second letter suggests *m* rather than *n*, and there is scarcely room for *y* after the *p*. Its spelling thus suggests the cuneiform name of the city, *me-im-pi* or *mi-im-pi*, rather than the Egyptian *Men-nuf*.

For the name Marduk-*šar-ušur*, A-G rightly notes that *šar* is usually spelled in Assyrian fashion *sar* instead of in the Babylonian manner as here, but he is misleading when he suggests that the *s* is almost gone. Only one stroke is missing, and the traces that remain could only be read as *š*.

*Line 11 (No. 13, v., l. 5, and No. 10, v., l. 4)*

A-G reads as "very probable" *bbym* as the first word, but aside from regarding the first *b* as a preposition he has nothing to offer with respect to meaning. A better reading is *bbys*. This may be the talmudic *bisq*, "kneading bowl," or "kneading trough," which may also identify the wooden object known in cuneiform texts as *bisu*.

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<sup>17</sup> Cf. *Palti*; *Piltay*; *Palt̄ēl*, *Plat̄āhu*, and *Plat̄āh*.

<sup>18</sup> D. D. Luckenbill, *The Annals of Sennacherib*, Col. II, l. 56; *Ancient Records of Assyria and Babylonia*, Vol. II, §239, p. 119.

<sup>19</sup> M. de Clercq, *Collection de Clercq* (Paris, 1903), II, 126 ff., Tablet C, ll. 1-2, 5 and 12, Pl. XXVI, 3A-F.

<sup>20</sup> G. Contenau, *Contrats néo-babyloniens*, Vol. II (1929), No. 193, l. 33.

<sup>21</sup> Cowley, *AP*, No. 37, l. 11.

## DARIUS AND XERXES IN BABYLONIA

GEORGE G. CAMERON

Problems connected with the chronology of the accession years of the false Bardiya, the two Nebuchadnezzars, and Darius have repeatedly been published in this *Journal*. Those connected with the accession of Xerxes have received ample publication elsewhere. New discoveries and a re-examination of the sources now make possible a clearer interpretation of these events.

## I. BARDIYA AND "TARZIYA"

The available evidence from Babylonia, supplemented by that from Egypt (see R. A. Parker's article in this issue), definitely confirm the traditional date of 522/21 for the accession year of Darius. Once this is recognized, it is obvious that all the material from Babylonia must fit in one way or another into the general picture. That it does so is clear from the following summary.

Herodotus, in Book iii, chapters 66–67, informs us that Cambyses reigned for seven years and five months. As others have shown, this figure is absolutely correct. The earliest document recording the sole rule of Cambyses<sup>1</sup> is dated on the twelfth day of the sixth month, 530 B.C.; from the Behistun inscription, § 11, we know that Bardiya revolted on the fourteenth day of the twelfth month (523/22); Cambyses reigned, therefore, six full months of 530/29 plus six full years plus eleven months of 523/22—a total of seven years and five months. A few scribes in Babylonia, as is to be expected, were unaware of Bardiya's revolt; others, of Cambyses' death. The last tablet dated to Cambyses, signed on the twenty-third day of the first month of the eighth year,<sup>2</sup> was written by such a scribe. But the scribe who wrote the first extant published tablet of Bardiya knew that the last month of the Babylonian year 523/22 was in itself Bardiya's "accession year"; therefore, he correctly dated a document written in the

<sup>1</sup> See W. H. Dubberstein, "The Chronology of Cyrus and Cambyses," *AJSL*, LV (1938), 417–19.

<sup>2</sup> See Olmstead, *AJSL*, LV (1938), 397.

next Babylonian calendar year to "year one, first month, day 19."<sup>3</sup> Another scribe, unaware that Bardiya's revolt began in the last month of 523/22, dated his tablet to "accession year, second month."<sup>4</sup> Other scribes were similarly confused; some continued to regard 522/21 as Bardiya's "accession year," while others regarded it (correctly) as his "first year."<sup>5</sup>

In § 13 of his Behistun inscription Darius says that Bardiya was killed on the tenth day of the seventh month. The last tablet dated to him is "year one, seventh month, first day"—nine days before his death. Herodotus iii. 66–68 relates that Bardiya was unmasked and killed in the eighth month of his reign; this, too, is accurate. Bardiya's reign included the last month of 523/22 plus six full months of 522/21 and part of the seventh month—a total of eight months.

One tablet, never published in full, would seem to indicate that a Tarziya (often identified with Barziya/Bardiya) still reigned in the eighth month.<sup>6</sup> Pinches, who gave us our first information about this text, stated the case as follows:<sup>7</sup>

The tablet 82-9-18, 360a,<sup>8</sup> which is a receipt for a certain amount of tithe for the month Marcheswan (October), paid to Takiš-Gula [sic!] (apparently a receiver of tithes at Sipar or Sepharvaim), is dated "the 11th day of Marcheswan, in the first year of Tarzia, king of Babylon and countries. ...." This Tarzia is apparently a variant for the more common Barzia.

The type-set signs given by Pinches read:

(araḥ) Araḥsamnu U<sub>4</sub>-11-KÁM  
MU ← KÁM Ḫ TAR-zi-ia  
šār TIN-TIR(ki) u KUR-KUR

That any scribe in Sippar, one month and one day after the death of Bardiya in the Nisaean Plain not far from modern Kirmanshah—and therefore only a few days' ride from Sippar—could still be dating tablets to him, even in a moment of absent-mindedness, is impossible.<sup>9</sup>

<sup>3</sup> Poebel, *AJSL*, LVI (1939), 123.

<sup>4</sup> *Ibid.*

<sup>5</sup> In addition to the list of known tablets dated to Bardiya (see Poebel, *loc. cit.*), Professor F. W. Geers noted the unpublished text BM 77436 which reads: (araḥ) Abu MU-1-KAM ḡar-zi-ia šār babilī šār mātāti (day and provenience unknown).

<sup>6</sup> See now Poebel, *op. cit.*, pp. 123, n. 7, and 138 f.

<sup>7</sup> *The Babylonian and Oriental Record*, I (1886/87), 54 f.

<sup>8</sup> Now BM 74635, according to Kugler, *Sternkunde und Sterndienst in Babel*, II, 395.

<sup>9</sup> See also Poebel, *op. cit.*, pp. 138 f.

Some other explanation is demanded. The one here proposed is so simple that, although this writer is unable to collate the original, he has no doubt that it is the correct one. Clearly the scribe intended to write

MU 𒉩 KÁM 𒄑 kám-bu-zí-ia  
"Year one, Cambyses"

but, by the commonest sort of haplography, wrote instead

MU 𒉩 kám-bu-zí-ia.

Pinches knew that MU-1-KÁM was the normal way of writing "year one," and expected the name of a king to follow. Perhaps a badly written *bu*<sup>10</sup> appeared to him as 𒄑 TAR; thus arose a "king" Tarziya, whose existence we must strongly doubt.

Striking corroboration of this explanation is found in the appearance of Taqish-Gula, named by Pinches as "a receiver of tithes" at Sippar. The name is quite common. There is hardly any reason to doubt, however, that the Taqish-Gula at Sippar named in the Pinches tablet is any other than the person of the same name who received offerings in this city for the *sattukku*<sup>11</sup> from the second year of Cyrus onward.<sup>12</sup> From the reign of Cambyses specifically, Taqish-Gula figures as the recipient of objects for this offering<sup>13</sup> on one tablet from the accession year, six from the first year,<sup>14</sup> four from the second year, fourteen from the third year, two from the fourth year, and two from the fifth year. Thereafter Taqish-Gula, in this capacity, is heard of no more.

## II. NEBUCHADNEZZAR III

The words of Darius, in § 16 of his Behistun inscription, indicate that Bardiya's death was followed immediately by the "revolt" of Nidintu-Bel, who called himself Nebuchadnezzar. It is now quite

<sup>10</sup> It is still more probable that the scribe actually wrote MU 𒉩 KÁM 𒄑, then had his attention diverted for a moment; when he turned back to his tablet he saw the KÁM and, without bothering to erase the vertical wedge, wrote immediately after it *bu*-zí-ia. The vertical wedge, followed by a somewhat crowded Neo-Babylonian *bu* (see the form of this sign as in Strassmaier, *Cambyses*, No. 60, rev. l. 1) could have been mistaken for 𒄑 TAR.

<sup>11</sup> Pinches' translation of the signs for *sattukku* at this period was "tax, tithe"; see his *Inscribed Babylonian Tablets* (London, 1888), pp. 16, 34.

<sup>12</sup> See Tallqvist, *Neubabylonisches Namensbuch*, s.v.

<sup>13</sup> *Ibid.*, supplemented by occurrences found in the files of the Oriental Institute's Assyrian Dictionary.

<sup>14</sup> Strassmaier (*Cambyses*, No. 69) mentions Taqish-Gula and is actually dated year 1, month 8, day 4, or one week earlier than the Pinches "TARZIYA" tablet.

clear that all tablets assignable to this period which belong to Nebuchadnezzar III are dated to the "accession year of Nebuchadnezzar."<sup>15</sup> The first tablet so dated has been thought to be one written on the seventeenth day of the seventh month—a week after Bardiya's death. One other tablet, however, appears to be dated to him at Sippar on the fourteenth day of the fourth month in the same year.<sup>16</sup> This cannot be correct, however, for it would signify either that Nebuchadnezzar III had already "revolted" at Sippar in the fourth month (unlikely, since Sippar acknowledged Bardiya in that month) or that this one tablet, dated "accession year," belongs to Nebuchadnezzar IV (also improbable, since all the documents dated to the latter are dated "year one").<sup>17</sup> A solution, though given here quite reluctantly, is that Strassmaier's copy is to blame. The Neo-Babylonian signs for the months Du'zu (fourth month) and Tashritu (seventh month) are similar; one vertical wedge, often written under two horizontals, is the chief difference.<sup>18</sup> If we assume that the tablet in question comes from the fourteenth day of the *seventh* month, instead of the fourth month, then all our available evidence checks. Bardiya was killed near Kirmanshah on the tenth day of the seventh month; the news could easily reach Sippar four days later, as the writer knows from experience,<sup>19</sup> at which

<sup>15</sup> Poebel, *op. cit.*, pp. 134 and 136 ff.

<sup>16</sup> Strassmaier, *Nabuchodonosor*, No. 1. Cf. Poebel, *op. cit.*, pp. 135, n. 51, and 139 (where it is cited as unpublished) with reference to Ungnad, *OLZ*, Vol. X (1907), cols. 464 f., who there states that tablets of Nebuchadnezzar III are dated from the fourteenth day of the fourth month of the accession year to the twenty-seventh day of the seventh month of his first year (Poebel, *loc. cit.*, n. 66, cites the last as being [unpublished] in Berlin). Ungnad refers, however, to his review in *ZA*, XIX (1905/6), 416, n. 1, where he assigns Strassmaier, *Nabuchodonosor*, Nos. 1 and (perhaps) 18 to this period. No. 1 is the tablet dated the fourteenth day of the fourth month of the accession year, and No. 18 is dated the twenty-seventh day of the seventh month of the first year! Poebel's table on p. 135, with notes 51 and 66 are, therefore, to be corrected; n. 64 is likewise to be struck out, for the tablet referred to is Strassmaier, *Nabuchodonosor*, No. 17, cited in Poebel's n. 63.

<sup>17</sup> That No. 1 in Strassmaier, *Nabuchodonosor*, belongs to this period (with Ungnad) seems clear from the occurrences in Sippar of Bel-etiř, *epiššānu*, from the tenth, twelfth, thirteenth, and fifteenth years of Nabunaid; the second year of Cyrus; the first, second, and fifth years of Cambyses; and the fifth year of Darius.

<sup>18</sup> See Poebel, *op. cit.*, pp. 135 and 139 f.

<sup>19</sup> The respective signs for these two months have confused others also (cf. Thompson, *Catalogue of the Late Babylonian Tablets in the Bodleian Library, Oxford* [London, 1927], pp. 8 [No. A 111] and 80 [Corrigenda], who writes of another date "Tammuz [or Tishri?]," and Dougherty, *GGCI*, Vol. II, No. 84, where the copy clearly shows Du'zu [fourth month] but in the "Catalogue of Tablets" on p. 60 this is read "seventh month").

<sup>20</sup> See also W. Hinz, "Das erste Jahr des Grosskönigs Dareios," *ZDMG*, XCII (1938), 146.

time Nebuchadnezzar III "revolted," and a scribe dated his tablet accordingly to "Nebuchadnezzar, accession year, seventh month, fourteenth day."<sup>20</sup>

This Nebuchadnezzar ruled, at least in Babylon and Borsippa, uninterruptedly through the twenty-first day of the ninth month (his last extant published tablet). Darius engaged him in battle five days later, and again on the second day of the tenth month. Four days after the latter event, presumably by the sixth day of the tenth month, or December 22, 522, Darius was acknowledged king in Babylonia.<sup>21</sup>

### III. NEBUCHADNEZZAR IV

In § 49 of the Behistun inscription Darius tells us that there was a second Babylonian "revolt" under a pretender "Nebuchadnezzar" (IV) while he was in Persia and Media—thus, presumably, sometime in the fourth month (of 521/20).<sup>22</sup> The first tablet of this period dated to "Nebuchadnezzar, Year 1"—and therefore assignable to Nebuchadnezzar IV—was written on an unknown day of the fourth month at Babylon.<sup>23</sup> Throughout the fifth month and the first day of the sixth, however, Sippar was held by Darius, and scribes dated tablets to him.<sup>24</sup> By the latter part of the sixth month, specifically on the twenty-fourth day, Sippar was in the hands of troops of Nebuchadnezzar,<sup>25</sup> who also occupied Babylon and Borsippa in the north and Uruk in the south throughout parts of the sixth and seventh months. Their control at Sippar was only temporary; Darius was acknowledged as ruler there on an unknown day of the seventh month, but Nebuchadnezzar once more on the twenty-seventh of that month.<sup>26</sup>

<sup>20</sup> Nebuchadnezzar III may even have revolted between the first day (last tablet of Bardya) and the fourteenth day of the seventh month.

<sup>21</sup> Note, however, Poebel's remarks in *op. cit.*, p. 134, n. 33.

<sup>22</sup> *Ibid.*, pp. 140 f.

<sup>23</sup> Strassmaier, *Nabuchodonosor*, No. 12. Contra Ungnad, *ZA*, XIX (1905/6), 416, n. 1, the Marduk-našir-apli descendant of Egibi in this tablet can scarcely be any other than Marduk-našir-apli, son of Itti-Marduk-balātu, descendant of Egibi of Strassmaier, *op. cit.*, Nos. 13 and 17, admittedly of this period; cf. also Apia, son of Suqaia, descendant of Lušu-anu-nur-Marduk of No. 12, with Bel-iddin, son of Suqaia, descendant of Lušu-anu-nur-Marduk, in Strassmaier, *Darius*, No. 236 (eighth year); also . . . mir, son of Bel-abbe-bullit of No. 12, with Itti-Bel-lummir, son of Bel-abbe-bullit, descendant of Sagdidi, in several Cambyses tablets (see Tallqvist, *op. cit.*, s.v.). See Poebel's table (*op. cit.*, p. 135) and correct accordingly by comparison with n. 16 above.

<sup>24</sup> Strassmaier, *Darius*, Nos. 16-19; correct Poebel's table (at n. 60).

<sup>25</sup> Strassmaier, *Nabuchodonosor*, No. 15.

<sup>26</sup> Strassmaier, *Darius*, No. 20; *Nabuchodonosor*, No. 18.

This curious picture of the fluctuating fortunes of the city as it recognized first one sovereign, then another, as troops of one or the other aspirants were quartered in its midst or camped just outside, is exactly what we should expect from these turbulent days. It has an exact parallel in ancient times in the struggle for the city Seleucia between two Parthian rulers, Phraates IV and Tiridates II, each of whom commemorated his all too brief dominance by actually coining money within the city's walls.<sup>27</sup> News of the capture of Nebuchadnezzar IV on the twenty-second day of the eighth month was doubtless received in Sippar with a sigh of relief. Babylonia, though now under the control of a foreign conqueror, was at peace, and the documents were thereafter dated to the reign of Darius.

### IV. XERXES AND THE BABYLONIAN REVOLT

For thirty-five years Darius capably governed Babylonia. From his thirty-sixth year come tablets which bear witness to his continued control through the fifth and sixth months and into the seventh.<sup>28</sup> His death, therefore, can with considerable probability be placed in the seventh month of 486, which began on October 12.

It has generally been assumed that Babylonia revolted once more either just before his death or when news of it arrived in the lowlands. Tablets dated to the accession years of Bel-shimanni, Shamash-eriba, "Akshimakshu," and "Shikushti" have been accepted as bearing witness to this revolt.<sup>29</sup>

Now it is quite true that Xerxes' appointment as legitimate heir to the throne, before the death of his father, passed over several other sons, as he himself declares,<sup>30</sup> and that this situation might well have provoked

<sup>27</sup> N. C. Debevoise, *A Political History of Parthia*, p. 137, n. 45.

<sup>28</sup> The tablets from the last three months of the thirty-sixth year are:

Month	Day	Provenience	Reference
V	5	Babylon(?)	BM 33,966, cited in <i>Guide to the Nimroud Central Saloon</i> . . . British Museum (1886), p. 117, No. 96, and <i>Guide to the Babylonian and Assyrian Antiquities</i> (3d ed., 1922), p. 158, No. 345
V	9	Borsippa	VS. VI, No. 166 (cf. No. 177)
V	28	Borsippa	VS. III, No. 164
VI	13	Dilbat	VS. III, No. 165
VI	19	Dilbat	VS. V, No. 110
VII	27(?)	Borsippa	VS. IV, No. 180

<sup>29</sup> Prášek, *Geschichte der Meder und Perse*, II, 112 and 148 ff.; R. W. Rogers, *History of Ancient Persia* (New York, 1929), pp. 146 f.; Ungnad, *OLZ*, Vol. X (1907), cols. 464-67.

<sup>30</sup> Herzfeld, "SAOC," No. 5; last published, *Altpersische Inschriften* (Berlin, 1938), No. 15, pp. 35-38.

revolts both within Persia and elsewhere. It is also true that he himself refers to the revolt of an unnamed country early in his reign.<sup>31</sup> His statement in itself is not, however, unequivocal.<sup>32</sup> Disaffected elements in Egypt or in Asia Minor, instead of in Babylonia, could be referred to by the words "Among those lands which are enumerated above there was (one which) was restless." Furthermore, there is no evidence to show that Xerxes carried out in *Babylonia* his assertion that, "where formerly the daiva's were worshipped, there I worshipped Ahura Mazda. . . ." Thus, although the present writer believes that Babylonia was intended, it must be admitted that these inscriptions themselves give no proof for this belief and cannot help us in dating the revolts.

On the other hand, the available evidence shows that Darius' chosen heir, Xerxes, was accepted immediately in Babylonia. The last tablet of Darius in the seventh month is followed without a break by an accession-year tablet of Xerxes in the eighth month (Arahsamnu 22 = December 1, 486). Other extant tablets demonstrate that Xerxes' reign was uninterrupted throughout the balance of his accession year and all of his first year.<sup>33</sup>

<sup>31</sup> Herzfeld, *Altperzische Inschriften*, No. 14, pp. 27-35; see also Kent, *Language*, XIII (1937), 292-305, and *JAOS*, LVIII (1938), 324 f., for the Old Persian version, and Weissbach, *Symbolae Paulo Koschaker dedicatae* (Leiden, 1939), pp. 189-98, for the Elamite version.

<sup>32</sup> Contra Hartmann, *OLZ*, Vol. XL (1937), cols. 145-60; esp. cols. 158-60.

<sup>33</sup> The tablets of Xerxes from Babylonia are:

Year	Month	Day	Provenience	Reference
Acc.	VIII	22	Borsippa	<i>VS</i> , V, No. 117
	IX (or XII?)	11	Borsippa	<i>VS</i> , VI, No. 177 ("Akshimakshu")
	X	7	.....	Strassmaier, in <i>Actes du huitième Congrès international des Orientalistes</i> , II (1893), Section Sémitique (B), after p. 283, No. 16
	X	22	.....	<i>Ibid.</i> , No. 17
	XI	9	Borsippa	<i>VS</i> , VI, No. 178 ("Akshimakshu")
	XI	27	.....	Strassmaier, <i>Actes</i> . . . , No. 18.
	XII	21	Borsippa	"Shikushti" tablet; see below
	x	x	Bit-Zabiran(?)	Evetts, <i>Inscriptions of the Reigns of Evil-Merodach</i> . . . , Appen., No. 2
1	I	23	Babylon	<i>BE</i> , VIII, No. 119
1(1)	I	23	.....	Thompson, <i>op. cit.</i> , p. 13, No. A 124
	II	7	Sippar	<i>Guide to the Nimroud Central Saloon</i> . . . British Museum, p. 120, No. 104
	III	3	Borsippa	<i>VS</i> , IV, No. 191
	IV (or VII)	x	Babylon	<i>VS</i> , VI, No. 179
	V	20	.....	Thompson, <i>op. cit.</i> , p. 8, No. A 111
	VI	17	.....	Strassmaier, <i>Actes</i> . . . , No. 19
				<i>VS</i> , IV, No. 192

[Footnote 33 continued on page 321]

Furthermore, the assumed king "Akshimakshu" is none other than Xerxes himself;<sup>34</sup> the scribe or scribes who so wrote the royal name had obviously misheard it and would no doubt have been quite surprised to discover that historians of a later day would ascribe it to another than Darius' legitimate successor.

Likewise there was no Babylonian rebel named "Shikushti." The evidence for such rests entirely on a brief paper read by Pinches before the Thirteenth Congress of Orientalists meeting in Hamburg in 1902, a résumé of which was published in the proceedings of that body.<sup>35</sup> Therein he presented the formulas of two tablets, one dated to Bel-shimanni, the other to "Šikušti, king of Babyon, king of the lands." Bel-shimanni is known from other sources and must be accredited (see below). But Pinches himself was dubious about "Shikushti," for he says:

The . . . Persian royal name is . . . mutilated, the first character, *ši*, and the last one, *ti*, being all that is certain. Between these two characters are two others, which look as if they might be *ki-nim*, but the inclination at

[Footnote 33 continued from page 320]

Year	Month	Day	Provenience	Reference
	VIII	30	.....	<i>VS</i> , IV, No. 193
	X	29	Rimme (Borsippa)	O. Krückmann, <i>Neubabylonische Rechts- und Verwaltungs-Texte</i> , No. 98
	XII	3	Susa	<i>VS</i> , IV, No. 194
	XII	13	.....	<i>VS</i> , VI, No. 180
2	I	x	Sippar	Evetts, <i>op. cit.</i> , Appen., No. 3
	III	14(?)	.....	<i>VS</i> , VI, No. 181
	III	15	Sippar (?)	Revillout, <i>PSBA</i> , IX (1887), 238
	IV	6	Borsippa	Clay, <i>BRM</i> , I, No. 85
2(1)	VIIb	25	Dür-	<i>VS</i> , V, No. 118: see n. 40
3	II	2	Babylon	Strassmaier, <i>Actes</i> . . . , No. 20
4	IV	1	Babylon(?)	Evetts, <i>op. cit.</i> , Appen., No. 4
5(?)	I	22	.....	<i>VS</i> , III, No. 181
5	IV	2	Babylon	Evetts, <i>op. cit.</i> , Appen., No. 5
9	II	9	Babylon	Moore, <i>Neo-Babylonian Documents in the University of Michigan Collection</i> , No. 56
12	X	6(?)	Nippur	<i>BE</i> , VIII, No. 120
16(?)	V	26	Arahtu	<i>VS</i> , III, No. 185
16	VI	2	Borsippa	Krückmann, <i>op. cit.</i> , Nos. 173-77; also <i>VS</i> , III, No. 182
	VI	5	Kār-Tashmetum (Babylon)	<i>VS</i> , III, No. 183
	VI	10	Borsippa	<i>VS</i> , III, No. 184
16(?)	x	x	Arahtu	<i>VS</i> , III, No. 186 (cf. No. 185)

Over 110 Elamite tablets from Persepolis, now undergoing examination in Chicago or copied by the writer in Teheran, document all the years of Xerxes except 8, 9, 11, 13, 14, 17, and 21. This group of texts is from the Treasury.

<sup>34</sup> See also Ungnad, *OLZ*, Vol. XI (1908), Beihefte, p. 25; F. W. König, *Reallexikon der Assyriologie*, s.v. "Akšimakšu." The dates of the tablets bearing this name fit admirably into the accession year of Xerxes as can be observed in the preceding note.

<sup>35</sup> T. G. Pinches, "Notes upon a Small Collection of Tablets . . . Belonging to Lord Amherst of Hackney," *Verhandlungen des XIII. Internationalen Orientalisten-Kongresses* (Leiden, 1904), pp. 267-70.

which some of the wedges of the inscription are written suggests that some, which look like "corner-wedges," are in reality uprights, or even horizontals, and in this case a modification of the traces would be necessary, changing . . . . *ki* to . . . . *ku*, and . . . . *nim* to . . . . *uš*.

No copy is given, and the owner of the collection, Lord Amherst, died before the tablet could be included in a proposed second volume of the Amherst tablets; the collection was sold in separate lots, and the location of this text at present is unknown.<sup>36</sup> Pinches' transliteration runs as follows:

*Bar-sip D.S. [ārah] Adari, úmu ešrāa-išten, [šattu] rēš lugal-nam-uš(?)-hu, Ši-ku(?)-uš(?)-ti šar Bābili D.S. šar mātāti.*

Apparent at once is the fact that *lugal-nam-uš(?)-hu* cannot stand. It is, of course, possible that *šarrūti* could be written LUGAL.NAM (if this is not another mistake of Pinches) instead of NAM.LUGAL.(LA). But, if so, then the royal name must begin either with the doubtful *uš* or at least with the *hu*; the latter sign must on the original be followed by a vertical wedge which Pinches could interpret as the male determinative. Anyone who takes the trouble to turn the transliteration back into the script of the period may see that the royal name can be read as one of the many forms of Xerxes' name. These forms vary from *Ak-ši-ia-ar-ši* and *Ak-ši-ia-mar-šu* (with their parallels) to *Hi-ši-i-ar-šu* (with its parallels) and *Ah-ši-ia-ar-šu*.<sup>37</sup> Pinches' *uš(?)-hu(?)* <<sup>m</sup>> *ši-x-x-ti* can easily be a misreading of partially illegible signs which are merely a minor variation of the latter form. His *uš(?)-hu(?)* is clearly *"Ah"*, which is followed by *ši*; *uš(?)-ti* at the end is obviously *mar-ši*, and the whole name is to be read *"Ah-ši-i(?)-mar-ši"*. Thus we may safely eliminate "Shikinimti" or "Shikushti" from the list of pretenders or claimants to the throne of Babylon. Since the witnesses' names "belong to the end of the reign of Darius, and are also found during that of Xerxes" (so Pinches), we may unhesitatingly add this tablet to the few already known from *Borsippa* dated in Xerxes' accession year.<sup>38</sup>

Through the accession and first years of Xerxes, therefore, there

<sup>36</sup> Pinches' prediction that his original paper would "probably appear" in *PSBA* seems never to have been fulfilled. It is to be hoped that he made a complete copy of this text and that his copy is now in the hands of E. F. Weidner, who inherited Pinches' copies of other tablets in the same collection; see *AJO*, XIII, Heft 1/2 (1940), 46.

<sup>37</sup> So written in Everts, *op. cit.*, Appen., No. 3; written *Ah-šu-mar-ši-i* and *Ah-ši-ia-mar-šu* in Strassmaier, *Actes . . .*, Nos. 16 and 19, respectively.

<sup>38</sup> See n. 33, above.

was no opportunity for any Babylonian to raise the standard of revolt. A revolt in the second year is likewise improbable. We know from Elamite tablets from Persepolis that the second year was an intercalary year, possessing a second Ululu.<sup>39</sup> As we shall see, tablets dated to the accession years of Bel-shimanni and Shamash-eriba, accredited rebels, come from months five and six (Abu and Ululu) and six and seven (Ululu and Tashritu), respectively. Now, if either of their revolts (particularly the latter) occurred in the second year, we would expect—in spite of our limited evidence—one of the tablets to be dated in that second Ululu. The fact that none is so dated is meager proof, but our suspicion that there was no disturbance in this year is corroborated by a Babylonian tablet of Xerxes' reign dated to a second Ululu of a year which must be the second year.<sup>40</sup> If, therefore, Xerxes held Babylonia in this month of the second year, it is at least unlikely that the land had revolted at all (or was in rebellion in the preceding and following months) in this year.

One or both of the revolts may, however, have occurred in any year of Xerxes after the second year (exclusive of the sixteenth). The third and fourth years are represented by only one tablet each; the fifth by two; the ninth and twelfth by one each. All the other years of Xerxes have no documentation whatsoever. This may, of course, be more or less accidental. What can hardly be an accident, however, is one of two changes in the royal titulary. In all the documents from the accession year through the first three months of the first year<sup>41</sup> Xerxes' title is "King of Babylon, King of the Lands," with a minor variation.<sup>42</sup> In the fifth month of the first year came word—no doubt by royal decree—that the title was to be changed. One scribe in that

<sup>39</sup> Nos. PT 4-158 and PT 4-745, copied by the writer in Teheran.

<sup>40</sup> VS, V, No. 118. The copied signs of the year-date are cross-hatched, indicating doubt on the part of the copyst Ugnad, and look like 4 above, plus 2 or 3, beneath. This would be an abnormal way of writing 6 or more in this period, however. Further, as will be made clear below, the title given Xerxes ("King of Persia, Media, king of Babylon and the Lands") is one used only from about the fifth month of the first year through the fourth month of the fourth year. The tablet can therefore hardly be dated after the fourth year, for it is not an unimportant document (cf. the witnesses), and its scribe would surely be aware of the correct royal titulary. Inasmuch as we know from other sources that Xerxes' second year actually had an intercalated Ululu (see n. 39), Ugnad's copy must represent a crowded 2-KÁM.

<sup>41</sup> Excluding VS, VI, Nos. 177 (accession year) and 180 (first year), and VS, IV, No. 192 (first year), which bear no title, and the two documents in Thompson, *op. cit.*, for which no title is given.

<sup>42</sup> "King of Babylon and Lands."

month wrote "King of Persia, Media," after Xerxes' name,<sup>43</sup> but thereafter, through the fourth month of the fourth year, the title is always "King of Persia, Media, King of Babylon and the Lands" (with minor variants). This alteration was merely an expansion of the first title and is scarcely worth emphasizing. The really marked change, and one which may well indicate royal exasperation with the refractory people of Babylonia, comes with the fifth year. Probably with the very first month of that year<sup>44</sup> "King of Babylon" is dropped from the royal titulary and is never again used throughout the balance of Xerxes' reign or in any of his successors'. The chief *Persian* title, "King of the Lands,"<sup>45</sup> though used earlier in Babylonia, now became standard. Its use for the first time in Xerxes' reign early in the fifth year is an argument, however weak, for dating at least one Babylonian revolt to the preceding or fourth year (482) just before Xerxes set out for Greece. Noteworthy is the fact that in Xerxes' army list Babylonia and Assyria are bracketed together,<sup>46</sup> indicating that each had lost its status as an independent unit. Further, when we bear in mind the fact that, according to some classical sources, Xerxes is connected with the theft of a statue of Marduk, with a revolt, and perhaps with a razing of Babylon *before his Grecian campaign* (481/80), then it appears quite likely that these events were bound up intimately with the revolts of Bel-shimanni and Shamash-eriba in the fourth year and an arbitrary change in the royal titulary by the fifth. When the revolts were put down, Marduk's statue was carried off so that no future rebel could "grasp the hands of Bel" and thus be recognized as the legitimate ruler of Babylonia.

The classical sources, unfortunately, do not entirely agree as to the time of the theft and the revolt. One passage of Herodotus (i. 183) merely states that, whereas Darius did not dare to carry off a coveted statue of Bel-Marduk, Xerxes not only succeeded in doing so but even killed a priest of the god in the process. Another passage (iii. 150 ff.) contains a folk story (often copied by later Latin authors) relating how Babylon revolted from Darius but, after a siege lasting

<sup>43</sup> Strassmaier, *Actes* ... , No. 19.

<sup>44</sup> VS, III, No. 181; cf. Everts, *op. cit.*, Appen., No. 5.

<sup>45</sup> Cf. Herzfeld, *AMI*, I, 15, for its possible Median origin.

<sup>46</sup> Herodotus vii. 63, which was called to my attention by Professor A. T. Olmstead.

over twenty-one months, was subdued through the machinations and self-mutilations of Zopyrus. The usually unreliable Ctesias<sup>47</sup> tells how Xerxes became king, visited Babylon to see the tomb of "Belitana," and then proceeded to Ecbatana, where he heard that Babylon had revolted and killed its commander, Zopyrus. Babylon, says Ctesias, was recovered by Megabyzos. Photius says of this version: "What [Herodotus] relates of Zopyrus is attributed by Ctesias (with the exception of a mule giving birth to a foal) to Megabyzos, son-in-law of Xerxes." Ctesias then specifically states that this revolt occurred and was put down before Xerxes departed for Greece.<sup>48</sup>

Unfortunately, likewise, there is nothing to indicate the specific year or years of Xerxes into which fall the "accession-year" documents of Bel-shimanni and Shamash-eriba.<sup>49</sup> All in all, there is no conclusive evidence regarding the time of the revolt. This much, however, appears evident: Xerxes followed Darius—in Babylonia at least—without any untoward event. There were no revolts against him in his accession year or in the first and second years which followed. His adoption of the title "King of Lands" by his fifth year is, in all likelihood, to be connected with the subjection of one or more revolts in Babylonia in the third, or, more probably, the fourth year.

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<sup>47</sup> Frags. 20-23 (ed. Müller; Gilmore's edition is not available). These sections of Ctesias' narrative are paraphrased and explained in Aelian *Var. Histor.* xiii. 3.

<sup>48</sup> Arrian *Anab.* vii. 17 (cf. also iii. 16) dates the sack of the Marduk temple after the return from Greece. No date is given in Strabo *Geogr.* xvi. 1. 5 or in Plutarch, *Moralia*, p. 173C.

<sup>49</sup> The Bel-shimanni tablets are:

Month	Day	Provenience	Title	Reference
V	10?	Borsippa	King of Babylon and Lands	See n. 35
VI	1	Dilbat	King of Babylon	VS, VI, No. 331
x	x	Borsippa	King of Babylon and Lands	VS, III, No. 180

The Shamash-eriba tablets are:

Month	Day	Provenience	Title	Reference
VI	25	Borsippa	King of Babylon	VS, III, No. 178
VI	x	Borsippa	King of Babylon, king of Lands	VS, III, No. 179
VII	21	Borsippa	King of Babylon	VS, V, No. 116
VII	22	Babylon	King of Babylon and Lands	ZA, III (1888), 140 f. and 157 f.
VII	23	Borsippa	King of Babylon, king of Lands	VS, VI, No. 173

## TWO INSCRIPTIONS OF ASHURNASIRPAL

It is not generally known that, in the possession of the Walker Art Gallery at Minneapolis, there are two inscriptions of Ashurnasirpal. These texts are inscribed on two limestone blocks or tablets; the first (No. 375) measures roughly 18 by 30 inches, and the second (Nos. 373+374) 18 by 36 inches. Both tablets are in an excellent state of preservation; the cuneiform characters are extremely clear, and it is only in the case of the second tablet that even a portion of the text has been obliterated (the right-hand edge of the stone is slightly worn).

The two texts are very similar to those published by Budge and King.<sup>1</sup> That on the first tablet is 23 lines in length and follows the version there given without significant variation except for the latter portion (rev., ll. 9 ff.) which is identical with the text published by Le Gac in *Les Inscriptions d'Aššur-naṣir-aplu III*, page 168, center.

The inscription on the second tablet, 24 lines in length, is quite similar to the fragmentary versions published on pages 166-68 of Le Gac's volume. The missing parts of lines 8-9 of those inscriptions appear as follows (l. 24): *ū rubē (pl) šá da-ra-te ēpuš(uš) ú-si-im-ši*, with which the Walker text ends.

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<sup>1</sup> *Annals of the Kings of Assyria* (London, 1902), I, 173-76: "Inscription upon Limestone Tablets Recording the Building of Ashur-naṣir-pal's Palace in Calah."

## FIFTIETH ANNIVERSARY CELEBRATION THE UNIVERSITY OF CHICAGO

In connection with the celebration of its Fiftieth Anniversary, the University of Chicago extends a cordial invitation to scholars and scientists to attend a series of symposia to be held from September 22 to 26, inclusive. "Approaches to Linguistics," "Interpretation and Criticism of Art and Literature," "Problems in Historical Materials," and "Archeology as a Tool in Humanistic and Social Studies" are some of the topics which will be of considerable interest to our readers. Among those who will take part are Am. Alonso, C. R. Morey, M. I. Rostovtzeff, E. H. Sturtevant, R. L. Engberg, H. Frankfort, A. L. Kroeber, R. H. Lowie, R. P. McKeon, W. L. Westermann, and H. R. Willoughby, of whom the first four named will receive honorary degrees in the course of the celebration. Everyone who wishes to accept the invitation is urged to send his name and address as soon as possible to The Director of the Fiftieth Anniversary Celebration, The University of Chicago.

## BOOK REVIEWS

KLEIN, WALTER C. *Al-Ibānah ḥan Uṣūl ad-Diyānah* ("American Oriental Series," Vol. XIX.) New Haven, 1940. Pp. xiii+143. \$2.00.

In his version of al-Ash'ari's *al-Ibānah ḥan Uṣūl ad-Diyānah (The Elucidation of Islām's Foundation)* Walter C. Klein presents this Moslem classic's first translation into a Western language. The translation's value is measurably increased by the Introduction—thirty-eight pages of skilfully condensed exposition. *Ijtihād* (p. 8, n. 19) is better rendered, literally, by "self-exertion" than by "effort, struggle." No one will question the assertion (p. 11) of the Umayyads' "unmistakable secularity," but one wonders whether the next statement—"Only one of them, 'Umar II, was a devout man"—is not too strong. Little mention is made of Zoroastrian influences in Islām and none of Manichaean: *zindiq*'s (p. 22) are defined only as "dualists, atheists." *bilā kayfa*, "the magic form of words" (p. 36), is rendered by "without further inquiry" (p. 24); "without *how*-ness" (i.e., "without further specification") or "without modality" would more nearly convey the sense to the "Christian theologian, non-expert to the resourceful intricacies of Moslem theology," whom the translation is intended to instruct. With the same Christian theologian in mind, one regrets that such a well-balanced book should be marred by the improper term "Muhammadanism" (p. 1) for "al-Islām."

The author fears lest his style "occasionally seem unsuitably colloquial" (p. 39); this reader found no passage where such was the case. The translation stands in lucid, straightforward prose, and the footnotes are both complete and instructive. al-Hajjāj b. Yūsuf, "the notorious Umayyad general" (p. 54, n. 124), would more aptly be called "viceroy" or "governor-general." There are few misprints: page 129, note 539, for Moth read Mother. In the index of Arabic words (p. 137 f.) *arsh* and *istiwa* (so given in Professor Macdonald's transliteration, p. 83, n. 306) should appear as *arš* and *istiwā* to conform with Dr. Klein's system. *ḥadīt* (p. 137) has lost its accent. Ibn 'Asākir's "Kitāb tabyīn kidb al-muftari fī ma nasaba ila-l-imām . . . al-Aš'ari" (cited on p. 25, n. 78, and again in the Bibliography, p. 39) should read "Kitāb . . . fī mā nusiba . . ."

The sum of such small mistakes in no way detracts from the value of the volume. This handy translation, buttressed by its Introduction, notes, Bibliography, and excellent indexes (of Arabic words, Introduction, and Qur'ānic citations) will be a ready tool for the hands of many. The translator well terms the *Ibānah* "an arsenal of arguments"; for recasting these weapons into dependable English he deserves the thanks of all students of the Moslem world.

L. V. THOMAS

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**BOOKS RECEIVED**

GLUECK, NELSON. *The Other Side of the Jordan*. New Haven: American Schools of Oriental Research, 1940. Pp. xviii+208+127 figs. and Frontispiece.

This compact volume is based on the lectures given by the author in this country from October to December, 1939. Most of the material has been presented before in various journals, for the most part in the annuals and the bulletins of the American Schools of Oriental Research, for which Dr. Glueck carried on his work as director in Jerusalem. The results of the author's explorations, the archeological finds, and their bearing on the period of the Hebrew advance into Palestine and the monarchy are of the utmost importance to the student. His chapter on Nabataean civilization includes the results of the work at Khirbet et-Tannur and gives a fine picture of that as yet too little known kingdom.—A. D. TUSHINGHAM.

GRANT, ELIHU, and WRIGHT, G. ERNEST. *Ain Shems Excavations (Palestine)*, Part V: Text. ("Biblical and Kindred Studies" No. 8.) Haverford, Pa.: Haverford College, 1939. Pp. 172+11 figs. +2 pls. in color, including Frontispiece.

With this volume of text to accompany the plates in *Ain Shems IV* (1938) is begun the definitive publication of the material collected during five field expeditions (1928-33) conducted by Haverford College. The material is well organized, separate chapters in Section I being devoted to stratification in general, defenses, and to each of the separate settlements on the site. In Section II the pottery is discussed stratum by stratum, and then the rest of the artifacts. Under the latter is included a very fine section by Emily de Nyse Wright tying the flint finds of Ain Shems with what is known of the Neolithic culture and periods immediately following, i.e., the Tahunian, before 4000 B.C., the Ghassulian of the fourth millennium, and the Canaanean: the culture of the historical period which begins as early as the second half of the fourth millennium.—A. D. TUSHINGHAM.

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